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ORIGINAL LECTURES.

INVESTIGATIONS RELATING TO THE ETIOLOGY AND PROPHYLAXIS OF YELLOW FEVER.

An Address Delivered before the College of Physicians of Philadelphia, April 21, 1888.

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GENTLEMEN: You are aware that I have been engaged during the past year in an investigation of the methods of inoculation practised in Brazil and in Mexico, by which it is claimed that protection against yellow fever is afforded. The subject is one of such great interest to the medical profession everywhere, and of such importance with reference to the sanitary interests of that portion of the United States subject to invasion by the disease in question, that Congress provided for an investigation of the methods of inoculation practised in Brazil and in Mexico, in the Act providing for the civil expenses of the Government for the year ending June 30, 1888.

Having been selected by the President to make the investigation referred to, in compliance with my instructions I first proceeded to Brazil for the purpose of investigating the methods of Dr. Domingos Freire, of Rio de Janeiro, and after my return from that country went to Mexico to make a similar research with reference to the value of the method of inoculation practised by Dr. Carmona y Valle. My detailed report was submitted to the President about a month ago, and by his permission I am now authorized to make public the conclusions reached. As I am about to go to Havana for the purpose of continuing my researches with reference to the etiology and prophylaxis of yellow fever, I am glad to avail myself of the kind invitation of the College of Physicians of Philadelphia, and to present to you, and to the profession generally, the conclusions which I have reached up to the present date.

These conclusions are given as follows in my report, above referred to:

"Facts relating to the endemic and epidemic prevalence of yellow fever, considered in connection with the present state of knowledge concerning the etiology of other infectious diseases justify the belief that yellow fever is due to a living microörganism, capable of development, under favorable local and meteorological conditions, external to the human body, and of establishing new centres of infection when transported to distant localities.

"Inasmuch as a single attack of yellow fever, however mild, protects, as a rule, from future attacks there is reason to hope that similar protection would result if a method could be discovered of inducing a mild attack of the disease by inoculation or otherwise.

"The hypothetical yellow fever germ, multiplying external to the human body in unsanitary places in tropical regions where the disease is endemic, or during the summer months in the area of its occasional epidemic prevalence, establishes infected localities, and susceptible persons contract yellow fever by exposure in these infected areas. We infer therefore, *a priori*, that the yellow fever germ invades the system by the respiratory tract, by the alimentary canal, or from the general surface of the body; and it should be found in the blood and tissues, or in the alimentary canal, or upon the surface.

"Another possibility presents itself, viz., that the germ multiplying in insanitary localities external to the body produces a volatile poison which contaminates the air, and that an attack is induced by the toxic effects of this potent chemical poison. The more or less prolonged period of incubation—two to five days—in numerous cases in which the attack has been developed after removal from the infected locality seems opposed to this latter hypothesis.

"In the light of what is known of the etiology of other infectious diseases, the hypothesis that the germ really finds entrance to the body of the person attacked and multiplies within it, is that which presents itself as most probable, and it hardly seems worth while to consider any other unless this is proved by a complete investigation not to be true. In the latter event we would have to consider the possibility of absorption through the respiratory tract of a volatile toxic agent, or through the skin of a poisonous ptomaine formed upon the surface of the body by a specific microörganism which does not itself penetrate to the interior.

"Naturally the attention of investigators has first been given to a search for the 'germ' in the blood of those attacked, and in the blood and tissues of the victims of the malady.

"*The researches made up to the present time have failed to demonstrate the constant presence of any microörganism in the blood and tissues of those attacked.*

"*My own researches, recorded in the foregoing report, show that no such microörganism as Dr. Domingos Freire, of Brazil, has described in his published works, or as he presented to me in his yellow fever germ at the time of my visit to Brazil, is found, as he asserts, in the blood and tissues of typical cases of yellow fever.*

"*There is no satisfactory evidence that the method of inoculation practised by Dr. Domingos Freire has any prophylactic value.*

"*The claims of Dr. Carmona y Valle, of Mexico, to have discovered the specific cause of yellow fever have, likewise, no scientific basis; and he has failed to demonstrate the protective value of his proposed method of prophylaxis.*

"It is highly important, in the interests of science and of public health, that further investigations be made by more exact methods, which have been perfected since Drs. Freire and Carmona made their researches, and with which they were evidently not familiar.

The failure thus far to find a specific microörganism

in the blood or tissues, makes it desirable that a thorough research should be made with reference to the microörganisms present in the alimentary canal, for it is possible that in yellow fever, as in cholera, the disease is induced by a microörganism which multiplies in this situation. Additional researches are also required before we can say definitely that there is no germ demonstrable in the blood and tissues. Having exhausted our researches by the method of direct examination, and by cultures from blood drawn during life, it is highly desirable that various culture media should be inoculated with material taken, with proper precautions, from the various organs, and at the earliest possible moment after death.[1]

I cannot attempt to give you in detail, at the present time, the evidence upon which the conclusions above stated are based, but it is recorded, *in extenso*, in my Report to the President, and will no doubt be published in due time. I shall, however, take advantage of this occasion to call your attention to some of the principal facts upon which I base the unfavorable opinion expressed with reference to the claims of Dr. Domingos Freire, of Brazil, and of Dr. Carmona y Valle, of Mexico. Both of these gentlemen, as you are aware, have laid claim to priority in the discovery of the specific germ of yellow fever, and both have practised inoculations with material supposed to contain their "microbe," having in view the production of a mild attack of the disease, and subsequent immunity as a result of this attack.

Dr. Freire has, however, made the greater number of inoculations and owing to his numerous publications and very positive assertions, his claims have received the most attention. My conclusion, that these claims are without scientific foundation, may seem to you almost incredible, in view of the extended researches which Dr. Freire has made in a locality where yellow fever is endemic, and of the very favorable statistics which he has published in support of the value of his method of inoculation. But I need scarcely remind you that the astonishing development of our knowledge relating to pathogenic microörganisms, which has occurred during the past decade, has been accompanied by numerous announcements of pseudo-discoveries in this field of research, and that much confusion has resulted from the premature publication of experimental researches made by enthusiastic investigators not familiar with the exactions of modern science, or with the exact methods by which alone security is offered against such pseudo-discoveries.

Dr. Freire insists in all his published works that his *cryptococcus xanthogenicus* is present in great numbers in the blood and tissues of yellow fever patients. In his principal work, *Doctrine Microbienne de la fièvre jaune*, published in 1885, he makes the following statement:

"The microbe xanthogenicus is a cosmopolitan; it does not select its domicile in any organ and has no preference for any organic liquid. We have encountered it with the same characters, the same opulence of proliferation, in the brain, in the muscles, in the liver, in the spleen, in the kidneys, in the lungs, in the blood, in the urine, in the bile, in the vomit, and even in the cephalorachidian fluid. However, it is necessary to establish a well-drawn distinction as to the blood; the blood of the general circulation shows itself much less charged with the microbes than the blood of the capillaries. Thus, if I could admit any preference on the part of the microbe xanthogenicus I would say that it pleases itself better in the blood of the capillaries, in the blood which bathes immediately the anatomical elements.

. . . . . .

"The occasion seems to us a favorable one in order to call attention to some indispensable precautions when the microbes of yellow fever are to be sought in organic solids and liquids. While it is extremely easy to perceive the presence of the microbes of yellow fever in the urine and the bile, for example, by placing a drop of these liquids upon a glass slide, covering it with a thin glass cover, and examining it with a power of 450 to 740 or 780 diameters, this proceeding cannot be employed when the blood is to be examined. If we proceed in this manner the globules will hide nearly all of the microbes, and the observer will wrongfully conclude that they are very rare in this organic liquid. Not only does the form of the microbe offer a certain resemblance to that of the red corpuscles, but these latter in adhering together envelop 'the microbian cells, and, on the other hand, cast upon the cells a jet of light which makes them disappear from the field of the microscope.'

"But if we dilute a little drop of blood in a pure solution of sulphate of soda and place it under the objective, the microbes become visible and will appear in considerable quantity,

"It is likewise necessary to make a preparation previously for the examination of the cerebral mass and of the muscles. They should be triturated in a sterilized mortar and mixed afterward with distilled water entirely deprived of organisms, filtered through fine linen which has been passed rapidly through the flame of an alcohol lamp, and afterward a drop of the filtered liquid should be placed upon a glass slide. If we withdraw a little piece of brain or of muscular fibre, even triturated, we will not perceive anything abnormal under the microscope unless it be the anatomical elements more or less deformed by trituration.

"It is not the same for the liver. It suffices to withdraw a bit of this organ and to crush it between two glass slides; upon observing it under the microscope we perceive at once a multitude of microbes. This is because in the muscles the microbes are lodged between the fibrillæ and in the substance which surrounds them; and in the brain they are found in the interior of the nerve cells which must first be destroyed by trituration in order that their parasitic hosts may become visible."

Having made extended researches in Havana in 1879, and during the past year in Brazil and in Mexico, I am able to assert most positively that no such microörganism as Dr. Freire has described is to be found in the blood and tissues of yellow fever patients. I have examined blood from numerous cases, drawn from the finger during life, both in the fresh condition, in preparations stained by various aniline colors, and by culture methods, and I have studied with great care a large number of thin sections of the liver and kidney, stained by the most approved methods, from a considerable number of typical cases of yellow fever, without having encountered the *cryptococcus xanthogenicus*.

As Dr. Freire asserts that his microbe is especially abundant in the capillary vessels, it should be easily demonstrated in thin sections of the various organs made *secundem artem*.

As a matter of fact, my researches show that, as a rule, no microörganism demonstrable by known methods of

[1] The writer went to Brazil and to Mexico fully prepared to make these experiments, but, unfortunately, was unable to secure any autopsies in either place, and the limit as to the time fixed by his orders made it necessary to return to the United States without having made these important researches.

staining, is to be found in such sections. But, here I must state that in certain cases microörganisms have been found. The one to which the most interest attaches is that described by Babes. This was discovered in 1884, in material sent to Paris from the laboratory of Dr. Lacerda, in Rio de Janeiro. At the time of my visit to Brazil, my friend, Dr. Aranjo Goes, gave me material from nine cases,[1] which had been preserved in Dr. Lacerda's laboratory since the epidemic of 1884, and after my return to Baltimore I examined sections stained by Gram's method, and with Lœffler's alkaline solution of methyline blue from all of these cases. I quote from my report as follows:

"In two of these cases I find in the capillaries of the liver and of the kidneys an organism which has been described by Babes and by Lacerda, who found it in material collected at the same time, and probably from the very same cases as those in which I now find it. This organism is a short bacillus, which occurs in chains, as seen in Fig. 2, Plate IV.

"In certain places, especially in the kidneys, it is found in the capillaries in great numbers, massed together; in other places it is distributed more sparsely, as seen in the figure." Careful examination of specimens stained with Lœffler's solution shows that the separate elements in these little chains vary considerably as to their length, and that the ends are more deeply stained than the centre. This appearance was no doubt observed by Babes, who first described the organism in question, but he has interpreted it differently. He says:

"The filaments appear united and homogeneous with an amplification of 600 diameters, but with a high power ($\frac{1}{12}$th hom. im., or No. 12 of Verick, which corresponds with the $\frac{1}{18}$th of Zeiss) one can assure himself that these filaments are composed of elliptical grains, almost cylindrical, arranged in pairs, forming little groups in which they are united by an intermediary pale substance. The filaments are thus composed of diplococci, or, if one wishes, of very short rods with terminal spores." (Op. cit., p. 522.)

Dr. Lacerda has described the organism referred to as in filaments which branch dichotomously, and believes this branching to be a constant and distinctive characteristic of the parasite which he accepts as the veritable yellow fever microbe. He is without doubt mistaken. The apparent branching of the filaments which he has described and drawn, and which he showed me in some of his preparations at the time of my visit to Rio, is due simply to the accidental juxtaposition of the torula-like chains. He is also mistaken in supposing that this organism is only to be satisfactorily demonstrated by Gram's method of staining. My friend, Dr. Goes, shared this belief at the time of my visit to Rio, but I demonstrated to him the facility with which the organism may be stained with a solution of methyline blue, upon sections which he made for me from material in Dr. Lacerda's laboratory. Since my return to Baltimore I have made numerous sections from the same material (two cases out of the nine) and find no difficulty in staining the organism present in the tissues, with methyline blue or with fuchsin. Dr. Goes also supposed that his failure to find this parasite in all of the tissues which he had preserved since the epidemic of 1884 was due to the fact that the tissues had been kept too long. He thought that it was most easily stained in recent tissues, and anticipated that when he had again an opportunity to make autopsies he would encounter this microorganism in the tissues. I shall await with interest his report of his recent researches.

As already stated, I have not been able to find this microbe of Babes in the tissues of six undoubted cases of yellow fever sent to me from Havana, and examined most carefully within six months of the date of the autopsies. Babes, himself, has renounced the idea that this microörganism bears an etiological relation to the disease under consideration. In the second edition of *Les Bacteries* he says:

"Since these researches we have had the opportunity to examine several series of sections from yellow fever: First, the liver and kidney of two individuals dead from this malady, collected by Dr. Alvarez, were examined in the Laboratory of Pathological Anatomy of the Faculty of Paris, without any bacteria having been found; second, material from three cases of yellow fever which Koch was kind enough to confide to one of us. In these three last cases, notwithstanding the most scrupulous research, and notwithstanding the advice of Koch, it was impossible to find the little chains in the brain, the kidneys, the liver, and the spleen. We must suppose, then, that in yellow fever, as in other infectious maladies, microbes are only found in the parenchymatous organs in certain cases, and not in all. The question whether these microörganisms really constitute the cause of the malady, or simply a complication, is consequently not resolved.

"We would remark that in view of the negative results attending Babes's more recent researches, and our own extended study of the tissues from six typical cases occurring in Havana, there is no good reason for supposing that the above-described microörganism bears an etiological relation to yellow fever. On the contrary, it seems probable that its presence in material from a limited number of cases occurring in Rio, is either entirely accidental, or is due to a secondary complication, perhaps to some form of septicæmia."

From what has been said, and from Dr. Freire's own account of his method of triturating fresh tissues to demonstrate his cryptococcus, the inference seems unavoidable that in his researches made by this method he has mistaken broken-up blood corpuscles and the granular *débris* of the tissue elements for microörganisms.

In his culture experiments he has, owing to a defective technique, encountered micrococci of various kinds, and one of these, which he brought back with him from Paris in an agar-agar culture, he presented to me as his yellow fever germ.

I brought a pure culture of this micrococcus with me to Baltimore, and have cultivated it during the past winter in various media. It does not correspond in its morphology, or mode of development, with the *cryptococcus xanthogenicus*, as described by Dr. Freire in his various published works, and, as stated, is not found in the blood or tissues of yellow fever patients.

In his principal work, published in 1885, Dr. Freire gives the following account of the morphology of his cryptococcus:

[1] In December last, having received a request from Dr. Frank Billings for some yellow fever material, I sent him a fragment of liver and one of kidney from one of these cases. It so happened that it was one in which the Babes organism was present, and Dr. Billings has since published an account of his examination of this material in a paper entitled, "Southern Cattle Plague and Yellow Fever."

"When we follow with care and attention the march of the development which characterizes the germs which produce yellow fever, we acquire the certainty that, commencing to present themselves under the form of little points almost imperceptible, they afterward gradually increase in diameter, until they attain considerable dimensions; so that these little beings, which, at the outset, had the aspect of very little grains of sand, not measuring more than 0.001 millimetre to 0.002 millimetre in diameter, arrive, little by little, to such a development that they reach the dimensions of 0.005, 0.007, 0.008 millimetre, and sometimes even more in certain conditions. When they have attained the adult age these cells are broken at divers points, and discharge their contents, composed of spores already formed, mixed with a viscous substance of a yellow color, composed of a pigment and protoplasmic substance, and of the liquids elaborated by the cells."

Even so recently as last year, in an address delivered before the Dosimetrique Society of Paris, Dr. Freire repeats this account of the mode of development of his cryptococcus; he says:

"Each adult cell is ruptured in one or several points, and allows to escape its contents, composed of germs which are to perpetuate the species, and two pigments, one yellow, destined to infiltrate all the tissues, and to produce the icterus color which has given name to the malady; the other, black, insoluble, and destined to be carried along by the circulatory current, producing either capillary obstructions, or blood stasis in the parenchyma of the organs."

Now this mode of multiplication is not known among the bacteria, and does not occur in the micrococcus which Dr. Freire placed in my hands as his yellow fever microbe, which multiplies by binary division, and does not differ in its morphology from a variety of microorganisms of the same class, which are extremely common in all parts of the world.

According to Dr. Freire's statement, the agar culture brought by him from Paris came originally from the blood of a patient with yellow fever at the point of death.

Now, as heretofore stated, I have demonstrated by the microscopic examination of numerous specimens, that, when proper precautions are taken, no microörganisms are found in blood drawn from the finger of yellow fever patients, and that no development occurs in culture media inoculated with such blood. I infer, therefore, that the micrococci, etc., found by Dr. Freire in his cultures, are due to the accidental introduction of microorganisms from without, and especially from the surface of the finger during the collection of the blood. I may remark here that Dr. Freire's cultures in liquid media, which were made before he went to Paris, and which were placed in my hands for examination, were all impure, and contained several different microörganisms. I suppose that the micrococcus brought back from Paris was isolated while he was there from one of these impure liquid cultures, for I found no evidence that solid culture media had been used in his laboratory in Rio prior to the date of his visit to Paris.

Nor did he use, to any extent at least, the well-known methods of staining bacteria which are recognized by bacteriologists as so essential in the study of these minute microörganisms. This is shown by the following quotation from his address, delivered in Paris, to which reference has already been made, he says:

"We know that in order to color a microbe it is necessary first to kill it and then to wash the little microscopic cadaver, by means of reagents possessing the power of dissolving all matters foreign to its skeleton. At the outset I applied myself to the study of the yellow fever microbe in a fresh state. I fed it with the best food—*les meilleurs engrais*—for the purpose of surprising the different phases of its evolution from its birth to its death."

"Nevertheless, in fault of other accusations, some authors have reproached me with not having colored my microbe. Alas! what a miserable objection. Is it necessary in order to affirm the existence of a microbe which swarms by millions in the urine, in the bile, in the blood, in the tissues, etc., is it necessary to mass them, to disguise them under a costume of carnival, in order to please certain microscopists? M. Pasteur has never colored his microbes, and nevertheless every one admits the existence of the bacillus of charbon, of the corpuscles of pébrine, of the micrococcus of fowl-cholera, etc.

"Do not think, gentlemen, that I fear the application of coloring processes to the search for the microbe of yellow fever. Far from it. In order to show you that the criticism which I have just made is not due to prejudice, I will say to you that such processes have recently been employed upon the yellow fever microbe with complete success."

A character upon which Dr. Freire insists, even in his address delivered in Paris in April, 1887, is the formation of two kinds of pigment, one yellow and one black. From the first he has affirmed that the black color of the characteristic "black vomit" is not due to the presence of blood changed by the acid secretions of the stomach, as has been generally believed by those physicians who have studied the disease, but that this color is due to a pigment produced by his cryptococcus. Now, I have had the coccus which he gave as his yellow fever germ under cultivation in various media, during a period of several months, and no black pigment has been produced. On the contrary, the colonies in Esmarch tubes, and stick cultures in gelatine, or in agar all have a milk-white color. In morphology this micrococcus does not differ from some well-known and widely distributed species. It liquefies gelatine quite slowly, grows readily at a temperature of 18.20° C. (64.4°–68° F.), and, as stated, forms no pigment.

The supposition that the microörganisms present in Dr. Freire's blood cultures, and in those of various other observers who have discovered yellow fever "germs" came from the surface of the body and not from the blood is sustained by recent experimental researches upon the sterilization of the hands, made by Kümmell[1] and by Fürbringer.[2]

These experiments show that it is not an easy matter to destroy all microörganisms upon the surface of the body by means of a disinfecting solution, and that a simple washing with bichloride solution of $\frac{1}{1000}$th does not usually insure sterilization of the hands. Fürbringer, after repeated experiments, recommends the following procedure: 1. Remove all visible dirt from the nails. 2. Scrub the hands with soap and water by means of a brush. 3. Immerse them for one minute in strong alcohol—at least eighty per cent. 4. While still wet im-

[1] Wie soll der Arzt seine Hände Desinficiren? Deutsche med. Wochenschr., 1886, No. 32, p. 555.

[2] Untersuchungen und Vorschriften über Disinfection der Hände des Arztes, etc. Wiesbaden, 1888.

merse them for one minute in a two per cent. solution of mercuric chloride.

Less thorough treatment did not in Fürbringer's experiments absolutely insure sterilization. In the case of patients in hospital, the difficulty is often increased by the fact that their hands are horny and begrimed with dirt which can only be removed by long scrubbing.

With reference to Dr. Freire's experiments upon animals, I can only say that those performed in my presence failed entirely to demonstrate that the micrococcus brought from Paris had any specific pathogenic power, and that I find no satisfactory evidence in the record of similar experiments contained in his various publications that it has ever transmitted yellow fever to rabbits or to guinea-pigs, as claimed.

I shall quote here a single experiment in which a fatal result occurred, and in which, as usual, Dr. Freire ascribed death to yellow fever resulting from the injection, notwithstanding the fact that the material injected (blood) had been subjected to a boiling temperature for some minutes. My own numerous experiments show that all known micrococci are quickly destroyed by a temperature much below the boiling point of water.[1]

Dr. Freire's account of this remarkable experiment is as follows:

"We must note that the microbe of yellow fever offers a remarkable resistance to heat. The following experiment furnishes a demonstration of this:

"On the 17th of April, 1883, we subjected to ebullition for several minutes, a gramme of blood containing the microbes. We injected it afterward into a guinea-pig, which had a temperature before the experiment of 38.5° C. (101.3° F.) in the axillary region. The temperature followed this march the following days:

April 18th	39° C. (102.2° F.)
" 19th	39°
" 20th	39.1°
" 21st	39°
" 22d	38.7°
" 23d	37.4° C. (99.3° F.)

"The animal died during the night of the 23d. Its autopsy showed the characteristic lesions.

"It is necessary to push the temperature beyond 200° C. in order to destroy the toxic energy of the microbe. As we have seen, its virulence resists simple ebullition. The microscope has shown that notwithstanding the boiling the microörganisms retained their ordinary form and continued to execute all of their movements, a proof of their complete vitality. (Op. cit., p. 217.)

"I may remark here that the micrococcus presented to me by Dr. Freire as his yellow fever germ is killed by exposure for ten minutes to a temperature of 60° C., and that it has no proper movements."

Dr. Freire's protective inoculations.—Having demonstrated that Dr. Freire's claim to have discovered the specific cause of yellow fever is without scientific foundation, it may be thought that no further demonstration is required in order to show that his preventive inoculations are without value; for these inoculations are said to have been made with cultures containing the attenuated microbe of yellow fever. These inoculations have, however, been made upon so large a scale, and the statistical results, as presented by Dr. Freire, appear so favorable to his method that it becomes necessary to analyze these statistics; and if, as he claims, they establish the fact that the mortality from yellow fever is very much less among those who have been inoculated by him than among non-inoculated persons exposed in the same way, we shall be obliged to concede the value of his method, although the rationale of the protective influence may not be apparent. In my detailed report I have reviewed at length Dr. Freire's statistics in the light of the facts developed by my personal researches in the city of Rio de Janeiro where the inoculations were made. I cannot attempt to bring the evidence before you at the present time, and I have already stated to you my conclusion with reference to the matter. In support of this conclusion I shall, however, quote a few extracts from my report.

In 1884, Dr. Freire inoculated 418 persons whose names, ages, place of residence, and length of residence in Brazil are given in an appendix to his principal work, published in 1885. In regard to the evidence afforded by these inoculations, I have written as follows:

"We remark, first, that in selecting foreigners and preferably those who had recently arrived in Brazil for his first experimental test as to the efficacy of his protective inoculations, Dr. Freire has given evidence of his confidence in the method of prophylaxis proposed and of an honest desire to demonstrate its value, and nothing could be more fair than his full publication of the names and of the essential facts with reference to these persons.

I must object, however, to his including in his statistics the names of thirty-seven persons residing in Vassouras, a village, which is some fifty miles distant from Rio de Janeiro. Even if these persons had occasion to visit Rio during the epidemic season, as is stated by Dr. Freire, it is probable that they would remain as short a time as possible, and there is no evidence that they were fairly exposed to the epidemic influence. Moreover, if any of these persons had contracted yellow fever as a result of a visit to Rio their names would not appear in the mortality list of this city, but in those of Vassouras, which are not given. The latter objection applies also to fourteen persons among the vaccinated whose place of residence is Nictheroy, a town upon the bay of Rio de Janeiro, which is the capital of the province of the same name; two persons vaccinated at Tijuca, and one on board the bark "Flive," two at Pavina, and three at Serraria must also be excluded. This reduces the number of vaccinated persons within the city limits to 355; and, of this number a certain proportion no doubt left the city soon after being vaccinated, and before any exposure, worthy of consideration in a test of this kind, had occurred."

Dr. Freire admits that "during the epidemic season a great number of the vaccinated were attacked by the malady," but claims that these attacks were of a mild character; yet he gives us the names of seven vaccinated persons who died from the disease. This list has been added to by some of Dr. Freire's confrères, as will be seen by the following translation of a letter published in one of the newspapers of Rio, and bearing date May 5, 1887. This letter is signed by Dr. Aranjo Goes, at present a member of the Central Board of Health, and a gentleman whose statements are worthy of the fullest confidence:

"My letter to the Imperial Academy of Medicine having been published, it now behooves me to publish

[1] The Thermal Death Point of Pathogenic Microörganisms. Amer. Journ. of the Med. Sci., July, 1887, pp 146-160.

the statistics relating to the vaccinations on Morro da Viuva.

One fact seems to me to be definitely demonstrated, that is the *worthlessness of Dr. Freire's vaccination*, as is well known to the medical profession of this city.

A year ago I wrote the following:

"The want of skill which he displayed in his first experiments, the false conclusions which he has drawn therefrom, and the thoughtless precipitation with which he has hastened to make known incomplete results without accompanying them with a single qualifying remark vitiate all the methods to which he may hereafter resort to corroborate his statements." (*Journal do Commercio*, April 20, 1883.)

"The mortality among the persons vaccinated on Morro da Viuva furnishes one more proof that I was right in saying this, as I now proceed to demonstrate.

"There were vaccinated in this district sixty persons.

"Sixteen removed shortly after the commencement of the epidemic, and forty-four remained exposed to its influence. Of these twenty-two had yellow fever, nine of whom died.

"The following is a list of the vaccinated persons who had the fever, the names of those who died being marked with an asterisk:

"*Antonio de Oliveira.
"Antonio Bento da Silva.
"Albino Francisco Maia.
"Jose da Silva.
"*Joaquin Pereira da Souza.
"Joaquin Gomes de Azevedo.
"Joaquin Ferreira Tolho.
"Jose Seabra dos Santos.
"Jose de Sonza Ferreira.
"Jose Gomes de Azevedo Junior.
"Jose Farinha.
"Manoel Joaquin Pereira Lopes.
"Manoel Gomes de Azevedo.
"*Manoel Antonio.
"Seraphim Goncalves Raymundo.
"*Manoel Simoes.
"Thome Simoes.
"*Manoel da Silva Alves.
"*Antonia Pereira Neves.
"*Joaquim Martins Pinheiro.
"*Joaquim Antonio dos Santos Cardoso.

"Consequently, of the 44 vaccinated persons who remained in the locality 22, that is 50 per cent. had the yellow fever!

"Of the 22 patients, 9 died, that is 40.9 per cent.!

"In the Jurujuba Hospital, which receives scores of patients already dying or in the third stage of the disease, the mortality is only 21 per cent.

After reviewing all of the evidence obtainable relating to the inoculations practised in 1883 and 1884, I say:

"The evidence above recorded seems to the writer to be convincing as to the complete failure of Dr. Freire's proposed method of prophylaxis as practised in 1883 and in 1884. We cannot, however, leave the question here, inasmuch as a modification of the method was adopted in 1884, and a large number of persons have been since inoculated by this modified method. Dr. Freire says in his report under consideration: 'I have employed in nearly all of the vaccinations the endermic method, and it is only recently that I have injected into twenty persons the same cultures by the hypodermatic method. . . . I shall hereafter give the preference to the last mentioned method, because one is more sure that the liquid employed goes to exercise its preservative influence.'

In 1885, Dr. Freire resumed his inoculations on a larger scale, but instead of selecting unacclimated strangers, those inoculated were for the most part natives of Brazil or Portugese who had lived for a number of years in Rio and who had passed through one or more epidemics. A considerable number of negroes were also inoculated and included in the statistical tables. With reference to Dr. Freire's statistics for the year 1885 I quote from my report as follows:

"Dr. Freire has omitted to state one very important fact with reference to the vaccinations practised during the period included in this tabular statement. The date of the vaccinations is not given. Fortunately I am able to supply this omission from his journal containing the names of the vaccinated, which he kindly placed in my hands during my stay in Rio. I find from this record that the inoculations were practised as follows:

January	392
February	342
March	611
April	139
May	273
June	813
July	481

"Now it is well known that June and July are months during which yellow fever does not prevail in Rio, and that in fact the month of May furnishes, as a rule, but few cases.

"The exposure even in an epidemic year amounts to very little during the months of May, June, and July, and may be considered practically *nil* in a year like 1885, when the whole mortality was only 278 in a city of 400,000 inhabitants. But Dr. Freire has included in his list 1294 persons who were vaccinated during the healthy winter months of June and July, and who presumably had been exposed during the preceding comparatively unhealthy months of January, February, March, and April. If these 1291 individuals were protected from an attack of yellow fever by the inoculation practised in June or July, what protected them from being attacked during the preceding months when yellow fever was prevailing to some extent?

"We remarked with reference to those persons selected for vaccination in 1883 and 1884 that 'in selecting foreigners and preferably those who had recently arrived in Brazil for his first experimental test of the efficacy of his protective inoculations, Dr. Freire has given evidence of his confidence in the method of prophylaxis proposed and of an honest desire to demonstrate its value.'

"In his inoculations practised in 1885 we no longer find any evidence of such selection, and so far as we can judge the vaccinated persons simply represent the average population of the city of Rio. It is well known that this population includes a large number of persons of foreign birth and especially of Portugese. The whole foreign-born population probably does not fall below 100,000 persons, but I have not been able to obtain any exact statistics with reference to this point. Dr. Freire vaccinated 2186 natives and 865 foreigners. Let us assume for the present that the 1760 persons vaccinated by him during the months of January, February, March, April, and May, were comparable so far as susceptibility to yellow fever is concerned with 300,000 of the population of Rio. In this estimate we exclude 100,000 of the population on the supposition that this number may have enjoyed immunity as a result of having suffered an attack of the disease, 278 deaths in a population of 300,000 gives less than 1 death per 1000, and there should not

have been over 2 deaths among the 1780 persons inoculated by Dr. Freire during the months of yellow fever prevalence.

"Let us look at the matter in another light. Dr. Freire gives the following table showing the length of residence in Brazil, of the foreigners inoculated in 1885:

For a few days	26
For a few months	71
From one year to one year and a half	69
Two years	107
Three years	98
Four years	126
Five years	103
On board ship (de passage)	9
More than five years	256
Total	865

"These figures also include the foreigners vaccinated in the healthy months of June and July, and those temporarily in the city (a few days 26, on board ship 9), but they will serve our present purpose, which is to call attention to the fact that 759 of the total number given had been in Brazil (and presumably in Rio) more than a year and a half, and had consequently passed through the preceding epidemic year (1884) without contracting yellow fever. If these persons resisted yellow fever during an epidemic, in which the number of deaths amounted to 1597, how can it be claimed that they are protected by a vaccination made in 1885, when only 237 deaths occurred, scattered about (sporadic) in a city of 400,000 inhabitants? the same argument applies with greater force to the 359 foreigners inoculated who had resided in Brazil for five years and above (five years 103; more than five years 256). Unless the list includes persons who had already suffered an attack of yellow fever, these individuals had passed through the epidemic of 1880 (1623 deaths) as well as that of 1883 and 1884, without contracting the disease, and we can hardly ascribe their immunity in the comparatively healthy year, 1885, to Dr. Freire's inoculation."

I pass now to the year 1886, during which Dr. Freire inoculated 2763 Brazilians and 710 foreigners, again including in his statistical tables those vaccinated after the epidemic season had passed. In reviewing these statistics, I remark as follows:

"We have quoted this last report of Dr. Freire *in extenso* in order to do him full justice by allowing him to state his own case. We shall now proceed to show that his statistics are fallacious, and that the percentage of mortality among the vaccinated, which he finds to be ten times less than among the non-vaccinated, results from a misuse of the statistical method and from a number of factors, which are favorable to Dr. Freire's statistics as he has stated them, but not to a fair test of his method of prophylaxis.

"In the first place, we would call attention to the fact that while during the comparatively healthy year, 1885, the immunity among the vaccinated of that year is said to be complete (see report of 1885), the number of deaths during the epidemic year which followed is stated by Dr. Freire himself to have been eight. Taking all of the vaccinated of the two years, and without making any allowance for the considerable number of persons vaccinated who had, no doubt, left the city before the epidemic of 1886 occurred, Dr. Freire with a total of 6524 vaccinated, and a total of 8 deaths, makes the proportion one per thousand. This is equivalent at the outset to an addition of 1476 persons to the number vaccinated, who being imaginary persons and not having been exposed to the epidemic influence simply aid in rounding up the general percentage of mortality in Dr. Freire's favor to the even figure of one per thousand. This is but one of many factors which go to make up this favorable showing. Reference to Dr. Freire's MS. Journals, which he kindly placed in my hands, shows that of the total number vaccinated during the two years, 4465 were vaccinated prior to the epidemic of 1866, that is to say, before the 1st of January, 1886. How many of these left the city before the outbreak of the epidemic, how many were only temporarily in the city when vaccinated, how many died from other diseases, I cannot say; but it is a significant fact that of the 3051 vaccinated prior to August, 1885, Dr. Freire has only one fatal case to report, while out of 460 persons vaccinated in January and February, 1886, he reports five deaths, a mortality of more than one per cent., which he gives as the general mortality among the non-vaccinated. This is not apparent from Dr. Freire's own statement of the case, but is nevertheless true, as I shall proceed to show. In his report which we have just given in full, he does not give the date of the vaccination of these individuals, but upon referring to his MS. Journal for 1886, I find that No. 3 of his list, Jose, son of Jose da Costa Vieira, was vaccinated February 12, 1886; No. 4, Paschoal Ruffino, on the 6th of February, 1886; No. 5, Henri Constance, on the 1st of January, 1886; No. 6, Fernando Argenteiro, on the 20th of February, 1886; and No. 7, Antonio Saraiva, on the 12th of February, 1886. The same MS. Record for 1886 shows that during these two months—January and February, 1886—the total number vaccinated by Dr. Freire was 460. That is to say, the mortality among those vaccinated during these two months was more than one per cent. On referring to the mortality lists of the city for the same two months, I find the total number of deaths to have been 369, which in a total susceptible population of 160,000 (Dr. Freire's estimate) would give a mortality of one in 436."

Time will not permit me to extend any further this analysis of Dr. Freire's statistics, and I must refer you to my complete report for additional details, and for an account of my personal investigations in Rio de Janeiro. Nor can I occupy any further time in an account of the inoculations practised by Dr. Carmona y Valle, of Mexico, and of his alleged discovery of the specific cause of the disease under consideration. A simple perusal of Dr. Carmona's published work is sufficient to convince any competent bacteriologist that owing to a defective technique and inexperience in bacteriological researches, he has fallen into serious errors of observation and of inference, and that his supposed discovery has no scientific basis.

From what has been said, it will be seen that the question of yellow fever etiology is still unsettled, and that it remains, in fact, just where it was left by the Commission sent to Havana in 1879 by the National Board of Health. The researches made by myself as a member of that Commission, showed that in yellow fever there is no microörganism present in the blood of the sick demonstrable by the highest powers of the microscope. This

conclusion is supported by my more recent researches by the method of cultivation. Before, however, abandoning all hope of finding a specific "microbe" in the tissues, I desire to make cultures from the various organs, obtained at the earliest possible moment after death. Unfortunately, the opportunity did not offer itself for this important experiment during my visit to Brazil and to Mexico last year, but I hope to have the desired opportunity in Havana during the present summer, and shall also give special attention to a search for the yellow fever germ in the alimentary canal, where it may possibly be located, as is the case in cholera. It is true that the clinical history of the disease does not especially point to this location, but in a research of this nature, it will not answer to reject any possible hypothesis, because of preconceived opinions; and inasmuch as the present state of science justifies a belief in a specific microorganism, as the essential agent in the etiology of this disease, it is imperative that the investigations be continued until success crowns our efforts.

ORIGINAL ARTICLES.

EXPLORATORY LAPAROTOMY.

By J. M. BALDY, M.D.,
OF PHILADELPHIA.

THE whole subject of exploratory laparotomy is daily assuming such increasing importance that it is becoming necessary to have some exact data from which to judge of the propriety of this procedure. There is little doubt in the minds of most operating surgeons as to the importance of a complete and intelligent exploration of the abdominal cavity in all cases of obscure abdominal troubles of a serious character, which threaten life, and which refuse, within a safe amount of time, to yield to medicinal treatment. The active hostility to operative procedures about the abdomen comes, in great part, from so-called surgeons, men who have either had little or no practical experience in these matters, or who have had some very unfortunate results. Again, a quiet but determined opposition is often to be found amongst physicians. It is hard for these gentlemen to overcome the dread of interference with the peritoneal cavity, a dread which has been so thoroughly instilled into the professional mind that it will take, I fear, many years of patient work to overcome it. Even in cases in which the diagnosis is easily made, and in which every moment lost is of the gravest importance, men still hang back from a surgical procedure, apparently in the hope that something almost supernatural will intervene to save the patient. Where a doubt rests on the diagnosis, this reluctance becomes almost a dread, and generally calls forth an ill-timed opposition, until eventually the operation must be made, only to be found to be too late, or perhaps it is not made at all.

The fact cannot be too constantly kept before the profession that an exact diagnosis in abdominal diseases is very often impossible. This is the expressed opinion of the great majority of abdominal surgeons to-day, and it is the exception to find one bold enough to claim otherwise. In the words of Thomas, "he who declares that he does not frequently err belongs to one of two classes: that of those who lack the intelligence to appreciate their shortcomings, or the courage to confess them, or to that of those who by *suppressio veri* or *suggestio falsi* are shortsighted enough to hope to deceive the community which watches their careers." When such men as Tait and Thomas boldly acknowledge our lack of diagnostic precision, it is time for us to lay off timidity and embrace every means at our command to overcome the difficulties of the situation.

If it were certain that a band was binding a loop of intestine down, and strangulating it, or that there was a purulent peritonitis, or one of a dozen other conditions present, even a physician would not hesitate to have the abdomen opened, and the strangulation relieved, the peritoneal cavity washed out and drained, or whatever else might be found necessary. The risk to life which the operation itself might bring would be unhesitatingly accepted by any intelligent thinker, and would not have very much weight against an operation which offered such positive chances of relief and cure. But when a case is presented in which there is a very grave doubt as to the diagnosis, especially when the patient seems to be in pretty good general condition, and with the hope of something turning up constantly leading one on, the temptation to wait becomes great. This is the more so because there is a decided feeling of uncertainty in the minds of the profession at large, excluding operating surgeons of experience, as to the amount of risk to life the patient will be exposed to from an operation *per se*, provided nothing of a dangerous character be found to exist after the operation has been made.

It is with the hope of adding something to the literature of the subject, which may at least be a beginning toward a final settlement of this question of danger, that I have collated the following statistics.

I am fully awake to the fact that conclusions drawn from statistics are often misleading, and can at best be but approximate; and yet withal they have their place and serve their object. I have been at some considerable trouble to make this list as reliable as possible, but realize that some errors have probably crept in, and that some cases must have been included which should not have been. However, I do not think there are enough of them materially to alter the results. I have not used any journal cases, excepting in such instances as the operator has directly referred me to them. I have excluded all such for the obvious reason that the case might be only one of many done by the same operator, and

it would be manifestly unfair to include his favorable and not his unfavorable ones, or *vice versa*. All cases used have been those of American surgeons; they have been obtained directly from the operator himself, and presumably include all such operations done by him during the past five years. Such cases only as have had the abdominal cavity opened and no operation or extensive separation of adhesions done inside the peritoneal sac, whether the diagnosis had been made before the operation or not, have been admitted, so far as I have been able to judge. Cases of simple irrigation or drainage have been included for the reason that they are often almost of necessity part of an exploratory operation, and do not complicate it to any great degree.

A careful analysis of the list develops many interesting features. There are in all 154 cases, representing the work of 46 operators, many of whom will be recognized as men of prominence. As reported, the list is classified as follows:

	Cases.
Carcinoma	3
Papilloma	5
Malignant	8
Sarcoma	12
Cancer	33
Pregnancy	3
Peritonitis, acute, chronic, suppurative	11
Peritonitis, tubercular	17
Uterine fibroids	16
Diseases of the uterine appendages	10
Diseases of the liver, gall-bladder, and their ducts	9
Cysts of kidney	2
Pelvic or iliac tumors	8
Pelvic abscess	2
Purulent fluid in peritoneal cavity	2
Abscess of ovary	1
Suppurating sinus	1
Cellulitis of broad ligament	1
Perimetritis	1
Acute micrococcal development	1
Hyperæmia of the pelvic peritoneum	1
No disease found	3
Penetrating stab-wound of abdominal wall	2
Penetrating gunshot wound of abdominal wall	1
Not stated	1

The total number of deaths is 25, making the general mortality somewhat over 16 per cent. At first sight this death-rate is appalling, and would naturally stop all talk of exploratory operations, but when we come to analyze further the deaths and their causes, this will be considerably modified. The list is considered in two divisions; malignant, 61 cases; and non-malignant, 93 cases.

Among the malignant cases age does not seem to have differed more than in malignant disease in other parts of the body. The mortality between the ages of twenty and thirty years is over 33 per cent.; from thirty to forty it drops to almost 19 per cent.; but between forty and fifty it again rises to over 33 per cent., only to drop back to 18 per cent. between fifty and sixty. Of the 55 recorded cases, 40 occurred between the ages of thirty and sixty years. The extremes were five and seventy-four years.

Of the 87 recorded cases of non-malignant disease, 71 occurred between the ages of twenty and fifty years, with a mortality ranging from 6 to 16 per cent. The mortality of those from one to ten, and above sixty years of age is from 50 to 100 per cent. I take it that these results are not very far from those of any other major operation.

The number of females far exceed those of males; the mortality amongst the males is, however, very much the highest. There are 61 recorded malignant cases; of these, 57 are females and 4 males. The female mortality is over 21 per cent., the male is 50 per cent. Of 93 recorded non-malignant, 83 are females and 10 are males; the female mortality is over 8 per cent., the male is 40 per cent.

At the time of the operation the condition of the patients varied greatly. Of 33 recorded malignant cases, 7 were in good or fair condition, 19 in bad, and 7 in very bad; of 76 recorded non-malignant cases, 25 were in fair or good, 34 in bad, and 17 in very bad condition.

The length of time that the disease had existed before the operation was in 32 recorded malignant cases, under one month, 1; from one month to a year, 23; over one year, 8. Of 62 non-malignant, 11 had existed under one month, 25 from one month to a year, and 26 over a year.

Of 32 recorded cases of malignant disease which recovered from the operation, 2 lived under one month, 15 from one to six months, 15 over six months; of 61 non-malignant, 2 lived under one month, 9 from one to six months, and 50 more than six months.

An analysis of the effects of the operation on the condition requiring it presents some very remarkable facts. 27 malignant and 52 non-malignant cases are recorded. Of the malignant, there was more or less improvement in 14, and no improvement in 13 cases; of the non-malignant, there was no improvement in 12, but there was improvement in 40 cases. The change for the better in some of these amounted to an absolute cure, and in many others it was very great, as well as permanent. This is all the more remarkable when it is remembered that nothing could be done for the patient's relief after the abdomen had been opened, but the wound had been closed with everything left as it had been found. This bears Tait out well in the statement he made some years ago, that the mere handling of the abdominal contents seemed in many cases to give relief. The modus operandi is, I think, beyond explanation, but the bare fact still remains, to which I can personally bear testimony.

14 malignant and 11 non-malignant cases died. Of the malignant, 1 died within a day, and 13 from one to seven days; of the non-malignant, 3 died

within a day, 6 from one to seven days, and 2 lived over a week. The causes of death in the malignant cases were:

	Cases.
Exhaustion	4
Peritonitis	2
Shock	1
Intestinal obstruction	1

with six cases unrecorded.

Of the non-malignant:

	Cases
Collapse	1
Shock	1
Tetanus	1
Peritonitis	2
Heart clot	1

with five unrecorded cases.

I have already stated that there were 25 deaths. If we examine these deaths we find that 14 of them were among the malignant cases, making a death-rate of 22 per cent. In other words, the mortality of the malignant is just twice as great as that of the non-malignant. Palmer has stated "that patients with internal cancer have little vitality and a small amount of shock is liable to shorten life, if not prove fatal." The results here obtained would seem to verify his statement.

Death in three of the malignant cases cannot be attributed to the operation, and probably if fuller particulars had been given in the other 11, most of them could be eliminated. As it stands, about 18 per cent. of these deaths were hastened by operation. Here, however, we must take into consideration that the death of all was only a matter of time, and possibly a very short time. The operation was left in most of them, as usual, until the last moment, and when the condition of the patient was, as recorded above, either "bad" or "very bad" in 26 out of 33 cases.

A much more pleasing picture presents itself in many ways when we consider the non-malignant cases, but here, too, we find a lamentable lack of determination and pluck on the part of some one—either the patient or the physician. Had an operation been undertaken earlier most of the deaths could probably have been avoided. There are in all 11 deaths, a mortality of over 11 per cent. Of these, I think we can, in all fairness, eliminate seven from the list of those in which death was hastened by the operation, leaving four cases in which the death was either hastened or caused by the surgical interference, a mortality of over 4 per cent.

Of these 4, 1 was a case of perimetritis, which died in nine hours; 1, a case of tubercular peritonitis, which died in ten days of acute peritonitis; 1, a case of hobnail liver, which died in forty-eight hours of shock; the last 1, a case of uterine fibroma, which succumbed in ten days to tetanus. The following are the 7 cases which I would eliminate:

1, Purulent peritonitis (dying); death in a few hours.

2, General peritonitis (very low); death in five days.

3, General peritonitis (almost in collapse); death in two days.

4, Suppurative peritonitis (very bad); death in four days.

5, Ruptured tubal pregnancy (in collapse); death in twenty-four hours.

6, Turbid serum in peritoneum (almost moribund); death in four hours.

7, Solid tumor of liver; death in two days.

This last operation was performed as a last resort at the express desire of the friends.

The whole number of deaths hastened by operation, in both malignant and non-malignant disease, is 15, a death-rate of 9 per cent.

There are 16 cases of uterine fibromata with only 1 death, and this from a cause which is as likely to complicate any other surgical procedure as the one under consideration. Furthermore, the prevention of tetanus is, as far as we know, entirely beyond our control. An operation to remove the ovaries or the tumor itself in the case of a uterine fibroma, which results in being able to do nothing, is probably as typical a case of exploratory laparotomy, in its results, as we can cite in judging of the danger of exploration. Another class which is just as typical and which occurs frequently, is disease of the uterine appendages. There are ten cases of this disease without a death. In fact, it seems to make little difference in this class whether the disease is removed or simply explored; one can be done with almost as much impunity as the other.

The penetrating wounds, 3 in number, all made good recoveries.

8 pelvic or iliac tumors, which could not be differentiated, all got well, as did also 2 of the 3 cases of pregnancy. The third was a ruptured tubal pregnancy, and the patient was almost dead when operated on. The 2 cases which recovered both aborted. 22 per cent. of the diseases of the liver, gall-bladder, and their ducts succumbed to the operation. I can only explain this on the ground of late operation.

The cases of peritonitis are among the most interesting and instructive on the list. I find 2 of chronic, 6 of general acute, and 3 of suppurative peritonitis, a total of 11 cases, with 4 deaths. 2 of these deaths were from suppurative and 2 from general acute peritonitis. 36 per cent. is a very large mortality, but the great death-rate of this disease where no operative interference has been practised must be remembered as well as the fact that all 4 deaths were almost certain at the time of operation, and would undoubtedly have occurred in any event. It is only another warning against the dangers of an expectant treatment in abdominal troubles. If we wait, and operate, as has been the custom too often, only as a last resort, we can never expect to render our results better than they have been in the past.

Probably the most remarkable class here recorded

is that of tubercular peritonitis. In a paper read before this Society last year, a case of tubercular peritonitis was reported, where the abdomen had been opened and death had followed. In commenting on that case, the reporter, on that occasion, a prominent physician of this city, said: "Incision should be avoided in tubercular peritonitis, unless the inflammation is purulent. Not only so, but where the tubercular process is present, even paracentesis should be practised only in those cases in which dyspnœa, from over-distention of the serous sac with fluid, threatens life. The danger of operative interference in tubercular inflammation of serous sacs is that the inflammatory process nearly always becomes purulent." In the discussion which followed, operative interference was generally condemned for various reasons. These opinions I have found to be pretty general among medical men. The facts are a decided contradiction of this, however, and in view of our rapidly accumulating experience, this subject will have to be very carefully reconsidered. Kussmaul reported 30 collected cases of operations on this disease to a recent Congress of German Surgeons. During the discussion of his paper 6 additional cases were reported, making a total of 36. There were in all 6 deaths, 4 of which were from general tuberculosis, and 2 were attributable to the operation. Many of the cases were encysted, and most of the patients were operated on under the supposition that there was an ovarian tumor or other cystic trouble present. I am now able to add 17 cases to this list, with but 1 death, the cause of death being general peritonitis. Thus we have a total of 53 cases with a death-rate of somewhat over 5 per cent. The 17 cases I have collected resemble in many respects those reported in Germany. They were for the most part cases of misdiagnosis or pure explorations for diagnosis. The manner in which they were dealt with varied. In some the ascitic fluid was simply evacuated, and the abdominal wound closed. In others a drainage-tube was inserted. Again, some were irrigated with water, or with water medicated with bichloride of mercury, and in still others iodoform was dusted into the peritoneal cavity. But there does not seem to have been any better result from one line of treatment than from another. The simple opening of the serous sac, with the evacuation of the ascites, probably supplemented by drainage, appears to be as beneficial as any. It is interesting to note that the patients not only did not die from the operation, but most of them had more or less permanent relief. Of the 16 cases of recovery, 5 are reported as cured, 4 as much improved, 2 as improved, and 4 as having no return of the ascites. In but 1 case did the ascites reaccumulate, and still the patient was alive sixteen months after the operation. Some of the patients have now remained well sufficiently long for us to be able to speak of their recovery with certainty. The amount of time now passed since the operation has been in 1 case over three years, in 2 cases over two years, in 5 cases over one year, the others varying from three to six months. The condition of 13 of the patients at the time of operation was either "bad," "low," or "apparently dying." Four only were in good condition. The case of death is reported as being very low, and a point of probable interest is that it is the only male in the series. It would seem then that operative interference in tubercular peritonitis is not attended with the risks attributed to it; but, on the contrary, is in a great majority of cases positively beneficial. There is a decided growing opinion among surgeons as to the propriety of adopting operative measures for the relief of this disease. Only within the past few months this subject was discussed in England by Mr. Knaggs and his colleagues with the result of reporting a number of additional cases, and the whole discussion taking a turn favorable to a further trial of operation.

To recapitulate, we have here a total of 154 cases of exploratory laparotomy, with a death-rate of over 16 per cent. By eliminating such cases of death as were very evidently not due to the operation itself in any way, the mortality becomes somewhat over 9 per cent. If we throw out the malignant and consider only the non-malignant cases, we find a death-rate of over 4 per cent. I think it fair to ignore the malignant and judge only from the non-malignant cases, because the operation in cancer is almost always a late one.

Even the mortality of 4 per cent. which we give to the non-malignant patients, is deceptive. For instance, of the 4 deaths, 3 are reported as being "very low" at the time of operation, 1 case of perimetritis, 1 of tubercular peritonitis, and 1 of hobnail liver. So in reality, although the operation directly hastened death, it probably only did so on account of the bad condition the patients were in, and should, therefore, not be taken into account in judging of what would be the result in a case operated on early in the attack, and which was still in good condition. What surgeon would expect to lose a case of simple exploration, of a patient suffering from a hobnail liver, from shock, under ordinary circumstances? The fourth case died of tetanus. This case was one of uterine fibroma, and was in very poor general condition, having suffered from the disease for over ten years. It is the only one I am inclined to give much importance to, and the only one which, after careful consideration, seems to me to have been a legitimate death from operation. Looking at the subject from this point of view, the death-rate falls at once to between one and two per cent., and this is, I think, about what it should be. Here is where Tait left it some years ago. In

his first 1000 cases of abdominal section, he had 93 exploratory with 1 death, and even this death was in a broken-down woman of sixty years, who died really of prolonged sickness after the anæsthetic.

These tables, I must admit, at first sight do not show what I should wish them to. But by considering only the non-malignant cases and by a fair exclusion of such deaths as cannot in justice be attributed to the operation alone, the results are not very far from the truth. I do not personally think there is any considerable amount of danger attached to a simple exploration of the abdominal cavity. A general mortality of over 16 per cent. as here shown can only mean that most of the cases had been undertaken when the patients were in such a low condition that the slightest shock would turn the balance against them. This is only too well shown to be true by reference to the condition of the patients before operation, as reported in this list. There is altogether too much waiting and expecting. If there is sufficient abdominal trouble of an obscure nature, to endanger life, which will not yield in a reasonable time to the ordinary methods of treatment, do not wait until the patient is worn out and in a condition in which no one would attempt any other major operation, but make a careful and complete exploration of the peritoneal cavity. It will be an exceptional case in which any harm will be done, and many an obscure case will be cleared up and many a life saved.

This line of treatment is growing more and more into favor with surgeons, and when the physician joins hands with the surgeon on this common ground the profession will no longer have to reproach itself for such a death-list as Treves reports for England, viz., over two thousand deaths every year from intestinal obstruction alone.

NOTE.—I wish to express my indebtedness to the following gentlemen for cases, many of which are as yet unpublished: Drs. William Goodell, 14; John Homans, 17; G. J. Englemann, 4; Paul F. Mundé, 3; H. P. C. Wilson, 4; W. W. Keen, 2; Henry O. Marcy, 8; C. D. Palmer, 5; R. B. Maury, 2; T. G. Morton, 3; R. S. Sutton, 1; Joseph Eastman, 1; Joseph Price, 7; Howard A. Kelly, 9; James B. Hunter, 6; John A. Wyeth, 3; R. F. Morris, 1; W. O. Werder, 1; William C. Wile, 1; John Scott, 1; Paolo De Vecchi, 1; George W. Porter, 2; W. B. Bontecou, 2; F. W. Rockwell, 1; J. R. Weist, 2; E. J. Beal, 1; Clinton Cushing, 4; T. A. McGau, 1; J. M. Barton, 1; Ap. Morgan Vance, 1; Donald MacLean, 1; Henry Beates, 4; John B. Deaver, 2; E. E. Montgomery, 5; Mordecai Price, 5; Harris A. Slocum, 1; W. C. Goodell, 1. I have received many others, but have been unable to use them, as they have been in one way or another somewhat more than simple exploration, at least as I have classified the list.

318 SOUTH SEVENTEENTH ST.

ACUTE EXOPHTHALMIA.

BY RICHARD H. DAY, M.D.,
OF BATON ROUGE, LA.

UNDER this heading, it is my privilege to report a very interesting case of sickness which recently came under my observation and treatment.

The subject is a colored boy, born December 6, 1872. His parents are still living and enjoy good health. The father is eighty years of age, his mother much younger. The boy is of medium size for his age, well built, and since infancy has enjoyed fairly good health. No constitutional taint that I can detect; yet his home surroundings, especially as to sleeping apartments, are very unfavorable and well calculated to engender systemic impairment and deterioration of the vital fluid.

I was requested to visit this boy, Abe W., on the morning of the 27th of January, 1888. The day before he had been out all day hunting in a cold, drizzly rain. When he returned home about dusk he was in his usual health, but fatigued from the day's hunt, and after eating his supper went to bed. Sometime during the night he became restless and complained of intense pain in his head. When daylight appeared his parents discovered that his left eye was very much swollen, and concluded that he had injured it the day before while hunting; but he insisted that he had not, and I am quite sure that he was correct, for there was an entire absence of any appearance or symptoms of traumatism. I found him with a moderate grade of fever, temperature 103° F. The bloodvessels about the temples were distended and hard. He complained of great pain through the temples and post orbital region of the left eye. The eye was pushed forward from its socket and protruded fully one-half to three-fourths of an inch beyond its normal site, presenting a strange spectacle, indeed, looking as though it would drop out at any moment. The lower lid was stretched as a tight cord across the lower segment of the sclerotica, while the upper lid was pushed forward and did not reach the upper corneal edge. The pupil was dilated, sight slightly impaired, but I could discover no cloudiness nor abnormal condition of the aqueous or vitreous bodies; though the globe seemed to be much distended and more conical than its fellow. I had no ophthalmoscope with which to examine the chambers and the posterior tissues of the eye, and hence am unprepared to say what abnormalities, if any, existed there, except as revealed by the symptoms and the subsequent history of the case.

Dunglison defines exophthalmia, "A protrusion of the eye from its orbit, occasioned by an abscess or tumor in the cellular texture of the orbit."

The suddenness of the attack and the rapidity with which the globe was thrust forward from its socket in this case, excluded the growth of a tumor, or phlegmon, or exostosis. I diagnosed the lesion to be excessive vascular congestion of the numerous and tortuous bloodvessels in the posterior orbit; and perhaps infiltration of serum or blood, from

rupture of some of the venous capillaries, in the connective tissue and Tenon's capsule behind the soft elastic fatty pad on which the globe rests, thus pushing the whole eyeball forward.

I visited my patient daily for six or eight days, watching his case very closely and with great interest, for the case was a novel one to me, and the first of the kind I had ever seen.

I kept him on small doses of calomel and ipecac for forty-eight hours and purged him with senna and epsom salts. Locally I instilled into his eye at intervals four or five drops of the following solution:

℞.—Sulph. zinc. . . . . gr. j.
Sulph. atropia,
Sulph. morphia . . . . āā gr. ij.
Aquæ destil. . . . . ℥ j.—M.

And a soft muslin bag containing infused tea leaves was laid over the eye. On the third day, his bowels having been well evacuated, I left off the calomel and gave him quinine in five grain doses every four hours, and applied behind his left ear a small blistering plaster, which drew a good blister. I could now begin to discern some recession of the protruded organ, but even up to the end of the eighth day it was still prominent above the bridge of the nose and the supraorbital brow. The local treatment was continued, and a tonic pill composed of quinine, pulvis ferri, āā grs. ij, pulv. digitalis j, was given three times a day.

When the eye had sufficiently receded to lessen the tension of the lower lid, so that I could depress or draw it down, I discovered an ecchymosed condition of the sclerotic, or what in common parlance is called a *bloodshot* appearance, which would seem to strengthen the theory of the extravasation of blood in the deep tissues in the bottom of the orbit, as the true pathological lesion.

I saw and examined this boy again on the 1st of March. The eye had quite receded to its normal position, and his sight, so far as I could judge, was not impaired, and he could distinguish the different colors. The globe had lost its distended and conical appearance, and I thought was somewhat flatter in its general aspect than the other eye.

I have looked in vain over my standard authors for light upon this unique case. They all treat of exophthalmic goitre, several cases of which I have seen in the course of my professional life; but these differ from my case in every characteristic, except only in the protrusion of the eyeball.

My patient had no affection whatever of the thyroid gland, no heart failure, and was not anæmic or chlorotic. The case was not chronic in origin, but was sudden and acute; was not protracted in character, but sharp and violent, and quickly yielded to medical treatment.

It occurs to me that within the last six or eight years I have read in medical periodicals, reports of cases somewhat similar in character to the one I now report, having occurred somewhere in the West, but I find it impossible to recall to mind the history of the cases or the journals in which they were published.

If any medical gentleman reading this report can supply the information, I shall be very thankful; or if similar cases have been witnessed by others and not yet made public, I hope they will be written up and published for my own enlightenment and the benefit of the medical profession.

A CASE OF SYMMETRICAL GANGRENE.

By M. A. BEADER, M.D.,
of Lyons, N. H.

The following case presents several interesting features and appears worthy of record:

The patient, a vigorous man, about forty years of age, appears in perfect health. His complexion is fresh and ruddy and his muscles extremely well developed. All the functions are normal, save only that he has a peculiar weakness causing a slight halting in his gait. He drags one foot slightly, as though unable to control its movements perfectly. He states that this defect became apparent about six years ago. It began soon after he had experienced "a stroke" or fainting fit, which he had attributed to drinking largely of cold water whilst overheated. He recovered soon and perfectly from this attack with the exception of this imperfection in his gait, which seems to be of a paralytic nature.

About eight months ago dark spots, resembling bruises but more clearly circumscribed, appeared just below the knee on both legs. They were painless but gradually became of a deeper color, the skin over them being smooth and glossy. They were not attended by heat or swelling, but in the early stages were indistinguishable from well-defined patches of purpura. At a later period ulceration began, the skin breaking down and leaving dark, purplish excavations. This process extended in an irregular way, whilst healing progressed imperfectly in the centre of the diseased area. The new skin over the parts that were healing was almost black and was very smooth and glossy. There was no hemorrhage at any time, no secretion of pus, and scarcely any odor. A slight watery exudation forming a glazing over the diseased surface was the only discharge apparent.

The ulcerations were of a gangrenous type without any active granulation process or formation of the red line, the entire course of the disease being extremely indolent. The margins of the ulcers were not elevated or hardened and did not present a punched-out appearance. Their color resembled very closely that of a sloughing bedsore. There were no varicose veins in the vicinity. No history of syphilis could be traced. Thoroughly well nourished, with sound teeth and healthy gums, the patient did not present any of the marks of scurvy. He had not taken ergot or mercurials, so far as could be learned. In fact, he had been in good health in every way with the exception of the imperfection of gait above mentioned.

Upon the first appearance of the spots he imagined

that they might be bruises caused by placing his knee against the stick while sawing wood. But when the disease appeared on both knees and persisted and increased month after month in spite of the greatest care, it became evident that it was not the result of injury. The lesions were symmetrically located about three inches below the patella on each leg. The diseased patches extended almost completely across the anterior portion of the leg, but the ulcerations were deepest just below the condyles of the tibia well around toward the sides of each leg. The right limb was considerably more affected than the left, the largest diseased portion being on that side and located on the external aspect of the tibia, a space between three and four inches in diameter being involved.

The symmetrical distribution on both limbs, as well as the superficial character of the ulceration and the way in which it spread on each limb, indicated that it could not have been the result of embolism or thrombosis. It is noteworthy that the lesions were in close relation with like portions of the same nerves in each limb. The patient had been under the care of a physician recently deceased, and could not give any trustworthy account of the treatment that had been employed, except to say very positively that it had been entirely ineffectual.

When I first saw him he had been using an ointment of some kind which apparently had been somewhat irritating. I directed a simple protective dressing of carbolated vaseline to be used after the ulcers had been very lightly cauterized with strong carbolic acid. Occasionally they were to be dusted also with iodoform. Under this treatment healing progressed slowly but better, according to the statement of the patient, than it had under the more irritating treatment that had evidently been employed previously.

At the outset, in this case, it is evident that there was a failure of the capillary circulation over the small spots first affected. There was no extravasation of blood, the spots remaining well defined, and the skin in their vicinity not undergoing any changes of color as it does when bruised. At this stage the lesions were simply purpuric. Subsequently the nutrition of the parts having failed, ulceration resulted and became extremely indolent. There was some attempt at repair in the central portions of the diseased areas, but it was very imperfect. The symmetrical distribution of the lesions on both limbs points to a central origin. The fact also that the patient had a paralytic disorder may be significant in this connection.

It would seem, in this case, that the vasomotor nerves controlling the circulation and nutrition of the parts affected were primarily at fault. The succession of events indicates the existence of a disorder of the same type as Raynaud's disease or symmetrical gangrene. I have not been able, however, to find any description of a case in which the lesions were situated in the particular locality which they occupy in the case under consideration.

MEDICAL PROGRESS.

The Ligature and Dressing of the Umbilical Cord.—JACOBI, in the *Archives of Pediatrics* for April, 1888, describes his method of treatment as follows: If the ligature be thin, it is liable to cut through the walls of the blood-vessels prematurely; if too thick, it may not suffice to compress them satisfactorily. It ought to be applied at a distance of from one and a half to two and a half inches from the abdominal wall. Not nearer, in order to avoid the effect of the immense muscular power of the umbilical arteries inside the abdominal cavity. A second ligature is placed about an inch from the first, and the cord cut between them. It is a good rule, which must surely be adhered to in every case of thick cord, to apply an additional ligature between the first and the abdominal wall, to avoid hemorrhage from the insufficiently compressed arteries, which may take place after the cord has commenced to shrink. The abdominal end of the cord is then wrapped up in a dry and soft piece of linen, lint, or cotton, placed on the left side of the abdomen, and fastened, by means of a soft flannel bandage, which is wide enough to cover the larger part of the chest and all of the abdomen, so as not to slip.

In wrapping up the end of the cord no oil must be used. Warmth and dryness favor mummification; moisture and exclusion of air, gangrene. This holds good also for the cord when it is separated from the living baby by an additional ligature, and in the dead. Thus, the former forensic axiom, that a dry cord proved life, which prevailed for decades after Meckel had demonstrated its fallacy as early as 1853, is absolutely worthless. Thus, fatty substances, and moisture of any kind, must be avoided as much as possible. Powdered subnitrate of bismuth, or oxide of zinc, or iodoform, or salicylic acid, one part with ten parts of starch, may be dusted round the insertion of the cord and over the stump daily. The latter application is not necessarily useless (from the point of view of antisepsis), for the separation of the cord is a gradual one, and not uniform through the whole thickness of the amnion and the three bloodvessels.

Creasote, Deeply Injected, in Phthisis.—DR. ROSENBUSCH gives an account in a Polish medical journal (*Przeglad Lekarsi* of February 4 and 11, 1888) of the cases of nine phthisical patients whom he treated by injecting creasote into the tissue of the lung. The results he obtained were excellent. The injections, which were given at intervals of two or three days, caused the cough almost to disappear and the quantity of sputum to diminish. In those cases which were not very far advanced the patients gained flesh, the dyspnœa and sweats ceased, the body weight increased, and frequently spots which had been dull on percussion cleared up. The temperature decreased from five to eight hours after the injections, the fall continuing at first for ten to twelve hours, a permanently normal temperature being subsequently registered. The injections were made into the affected portion of the lung, which was usually, of course, the apex. The in-

strument employed was an ordinary Pravaz syringe, with a needle from two to three inches in length. About eight minims (half a syringeful) of a three per cent. solution of creasote in almond oil were injected in each of two spots, the piston of the syringe being pressed down slowly. The patient was told not to breathe deeply, and after the injection to lie quite still for a few minutes. When the needle was not inserted deeply enough, some lancinating pain was apt to be produced by the irritation of the pleura. This, however, did not last long. No hemorrhage was ever observed after the injection, though in the case of one patient, who was subject to hæmoptysis, a slight coloring of the sputum was observed for a short time. If the injections were made into a cavity, or into one of the large bronchi, the patients said they could smell creasote when they coughed, and this substance could be detected in the sputum by chemical means. Of course, great care was taken to perform the operation under aseptic conditions, the skin, the needle, and the syringe being previously carefully washed with an antiseptic. The creasote used was the *creasotum e bitumine fagi*, or beech creasote. The writer promises a subsequent communication on the effect of these injections on the number of Koch's bacilli.—*Lancet*, March 31, 1888.

Putrid Bronchitis Successfully Treated by Inhalations of Corrosive Sublimate.—Korányi, of Buda Pesth, is quoted by Betz's *Memorabilien*, Band 32, Heft 7, as having treated successfully a case of putrid bronchitis, complicated by pleurisy and mitral disease, with inhalations of corrosive sublimate, as follows:

Hydrarg. bichlor. corros. . . .	⅙ gr.
Aquæ destill.	3½ ʒ.

Of this solution, five drachms were inhaled morning and night.

Six days of this treatment resulted in a diminution of fever, and in a great improvement in the character of the sputa and the general condition of the patient.

The Antiseptic Treatment of Smallpox.—Cavaillon is quoted by the *Gazette de Montpellier* of March 3, 1888, in his account of the following method of treating smallpox by local antiseptics:

He opens the pustules as soon as they are full of purulent matter, and presses out their contents upon cotton. The pustules are then touched with cotton dipped in the following mixture:

Benzine.	15 ♏.
Water	10 ʒ.
Pure alcohol	30 ♏.

This application is renewed until the crusts appear. When no more pus is found beneath the crusts an oily dressing is applied. If pus does not form freely the pustules are sprayed with the solution of benzine. After the twelfth day the following ointment is used:

Naphthol	75 grs.
Vaseline (with benzoin or oil of bergamot)	25 ʒ.

Idiosyncrasy to Antipyrin. — Taylor reports, in the *British Medical Journal* of March, 31, 1888, the following case: I administered to a lady on two different occasions eight grains of antipyrin for attacks of migraine, and on each occasion, very shortly after taking it, a tight feeling of constriction was felt across the chest, with a burning sensation in the pharynx. These symptoms were immediately followed by sneezing, by intense suffusion of the eyes, and by quantities of mucus flowing from the nose, giving her all the appearances of having a severe attack of coryza; there was also great irritation in the larynx, causing severe fits of coughing, but unattended with expectoration. After a quarter of an hour these uncomfortable symptoms gradually subsided. There was no urticaria. I followed it up on each occasion with an equivalent dose of antifebrin (three grains) which (with one repetition in the course of an hour on the first occasion, but which was not required on the second) completely relieved the severe hemicrania, as it has done on subsequent trials without using antipyrin at all. It appears, therefore, that antifebrin may be used equally with antipyrin in migraine as in febrile conditions, and may replace it with advantage where the latter disagrees.

An Accident from Antipyrin in the Treatment of Rheumatism.—Giguel, in the *Gazette Hebdomadaire* of March 23, 1888, reports the case of a woman of nervous temperament, the subject of herpes and suffering from rheumatism in the right shoulder. He prescribed forty-five grains of antipyrin, to be taken in three doses, during twenty-four hours. Ten minutes after the first dose was taken the patient experienced sensations of great irritation about the upper and lower jaws, sneezing followed, and persisted for a quarter of an hour. A sensation of pricking was felt at the thyroid cartilage, and the voice became hoarse and rough; the temples throbbed, and the nares were occluded by mucus. The painful shoulder, however, was free from pain, and could be moved freely. Five hours after the drug was taken only a slight roughness of the voice remained. A second dose of four grains was followed by the same effect, at the same time, in less degree. During the effect of the drug the side of the head and neck were intensely congested.

"Imitative Tetanus." — Dr. Vicente G. Gáunchez, writing in the *Revista Científica de la Universidad Central de Venezuela*, records a case of "neurosis tetanica," or "imitative tetanus," which occurred in a native of Caracas, a young man who had always enjoyed good health. The patient began to complain of pain in the calves of his legs which caused him considerable difficulty in walking. There was no fever, but the pain increased, and some contraction of the flexor muscles of the leg occurred, rendering the man incapable of walking. Indeed, on the second day of the attack he could not turn in bed without a good deal of pain. He was unable to sleep, and suffered from mental anxiety. It was then elicited that his brother, who lived at a little distance, was suffering from tetanus, and that the patient, who went frequently to see him, had appeared to be greatly affected at the sight of his brother's spasmodic attacks, and had declared that he himself would die of the same malady.

Dr. Guánchez, however, attributed the pains to rheumatism, which was common enough at the season when this occurred. He treated the man with anodyne lini-

ments and jaborandi internally. No effect was produced, as intense rigidity of the legs, simulating ankylosis, came on, the patient obtaining no sleep whatever and the pain being still very severe. A purgative was prescribed, but the trunk muscles became affected and clonic spasms began to show themselves. Bromide, chloral, narcein, and codeia were prescribed, but none of these arrested the rapid progress of the affection. The convulsions increased, opisthotonos and lastly trismus occurred, and the man died on the sixth day. In this case the mode of invasion was different from that usual in true tetanus, which commences by affecting the muscles of the face and chest; again, there were no remissions, as in ordinary tetanus, but the disease progressed steadily and more rapidly than is usual in tetanus. As to treatment, Dr. Guánchez, who has had some experience of ordinary tetanus, has found mercury and opium sometimes successful, and in one notable case the cure appeared to be due to the constant and careful administration of curare. —*Lancet*, March 31, 1888.

The Treatment of Retained Placenta and Membranes, in Breisky's Clinic.—BLOCH reports the treatment followed in Breisky's wards as follows:

1. Disinfection of the genitals.
2. Slow dilatation, with careful disinfection of the tents or instruments employed.
3. Disinfecting the interior of the womb, and intra-uterine examination by the finger.
4. Removal of retained membranes or placenta with polypus forceps, and intra-uterine disinfection.—*Centralblatt für die gesammte Therapie*, April, 1888.

Oertel on Diphtheria.—The *Lancet* summarizes Professor Oertel's conclusions regarding diphtheria, recently published, as follows:

In a few pages Professor Oertel alludes to the clinical and therapeutical bearings of these considerations. In the first place, the false membrane must no longer be regarded as the first pathological and pathognomonic sign of diphtheritic infection, and it is not surprising that energetic measures toward its removal are followed by fresh formation, since the morbid process is going on in the mucous membrane. Still, local treatment should aim at thorough disinfection of the buccal and pharyngeal cavities, which will at least prevent secondary infection of the stomach and intestines, and this is best effected by the frequent use of disinfectant spray. How far the disease is spreading may be judged by the degree of lymphatic gland swelling; and the degree of fever may bear upon the extent of general infection. When we know more of the influence of drugs upon ptomaines, in preventing their formation or hastening their conversion into innocuous material, we shall be better able to prescribe general remedies—such as quinine, mercury, or arsenic—with confidence; but meanwhile the chief hope lies in prophylaxis and the maintenance of the body in a state of nutrition, so that its cell elements may be able to resist successfully, and annul, the virus against which they have to contend. That this is possible is evident from what we know respecting other poisons and individual capacities of resistance.

In conclusion, Professor Oertel points out that the whole subject, from the first migration of leucocytes into the infected epithelium to the formation of necrobiotic foci in the spleen, proves the important part played by cells in the disease. It shows, he considers, that we must still go back to the cell theory if we would explain morbid action, and he puts forward his work as a contribution to Virchow's doctrine of cellular pathology, which is by no means so effete as some declare.

Formulæ for the Use of Digitalis.—HUCHARD, in the *Revue Gèn. de Clin. et de Thèr.* of March 22, 1888, gives the following formulæ:

Digitalis with bromides:

Tinct. digital.	♏ 30.
Potass. bromid.	ʒ 5.
Aquæ destill.	ʒ 9½.

Dose, one to three tablespoonfuls daily.

Digitalis and trinitrine

Tr. digital.	♏ 45.
Alcoholic solution of trinitrine, 1 to 100	gtt. 30.
Aquæ destill.	ʒ 9½.

Dose, from two to six teaspoonfuls daily.

Digitalis and iodide of sodium:

Sodii iodid.	ʒ 1.
Digital. pulv.	gr. 30.
Glycerin.	q. s.—M.

Ft. pil. 40 in num.
Sig.—Three or four pills daily, for five or six days.

The Effect of Cooking upon Starch.—In a graduation dissertation for the degree of M.D. in the Military Medical Academy of St. Petersburg, Dr. N. Butiagin gives an account of some experiments he has been carrying out with the view of determining the effect of cooking on the digestibility of starchy foods. He found that the activity of the saliva does not differ much amongst healthy persons, but that when people are badly nourished and weak, and especially when in addition to this they are suffering from disease of any kind, their saliva has a very perceptibly diminished power of dissolving starch. When starchy substances are subjected to prolonged cooking they become more easily digestible, and in this way compensation can be provided for the inactivity of the saliva of weakly persons. Thus, rice and peas were found to require three hours' cooking in order to render them as easily digested in the saliva of a badly nourished, nervous, hysterical woman (which had previously been found to possess only 88 per cent. of the normal activity), as they were with a single hour's cooking by the saliva of healthy persons; and generally it was found that in the case of weakly or diseased persons starchy food must be cooked twice or thrice as long as in the case of healthy persons in order that it might be equally acted upon. Again, when starch has been cooked for a long time there is less difference between the effect of healthy and unhealthy saliva upon it; this is specially remarkable in the case of millet—which, after one hour's cooking, showed a difference of 12.89 per cent. in favor of the saliva of healthy as against that of diseased persons, but after three hours' cooking a difference of 5.77 per cent. only.—*Lancet*, March 31, 1888.

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YELLOW FEVER INOCULATONS.

MANY recent bacteriological investigations have illustrated how sadly truth may suffer at the hands of incompetent, yet earnest, observers. Enthusiasm without judgment and observation without scientific training have been, and doubtless will continue to be reponsible for many errors, and it is unfortunate that a great part of the labor in any research is in separating the wheat from the chaff and determining just how much of the work done has been reliable.

The assertions and claims of Domingos Freire, of Rio Janeiro, that he had discovered not only the germ of, but a vaccine against, yellow fever, have naturally attracted special attention in this country, and we have, on several occasions within the past few years, noted the course of his investigations. The publication of his work on the disease enabled experts to determine how unsatisfactory were the methods which he followed, and the germ, *cryptococcus xanthogenicus*, which he described could not be classified with any known forms. More important by far, however, were his claims that he had found an efficient vaccine which had been tested in hundreds of cases. The interests involved were important enough to justify the President in sending DR. STERNBERG to investigate these methods, and we give in this issue the preliminary results of his studies as presented in a paper read before the College of Physicians of Philadelphia.

From the limited time allotted, Dr. Sternberg could not, unfortunately, secure any autopsies, and the chief value of his present investigation lies in the careful criticism and analyses of Freire's work. So far as we know, this has not before been attempted by any acknowledged expert. That Freire's claims have not stood the test will not surprise those acquainted with modern methods, and the extracts given in Dr. Sternberg's lecture suffice to show that his work lacks scientific precision, and is totally untrustworthy. It was quite possible that, missing the germ, he might still have discovered a protective virus, and the figures of successful vaccinations published within the past few years have given color to the belief that there was some truth in his statements. The figures presented, however, by Dr. Sternberg justify his conclusion that there is no satisfactory evidence that the method of inoculation has any prophylactic value. We have not the slightest doubt that in this opinion the leading bacteriologists of the world will concur.

Dr. Sternberg has been ordered to proceed to Havana for the further study of the disease, and we sincerely trust that with the elaborate and accurate methods now at command he may reach positive conclusions regarding this most formidable of tropical plagues.

BACTERIA IN THE URINE.

THOUGH the influence of bacteriology in furthering the study of hygiene and pathology is manifest to all, it is evident that the hopes of clinical medicine have not been fulfilled to the extent anticipated by many in the earlier days of that science. Thus, so far as practical medicine is concerned, examination of the sputum for tubercle bacilli is about all that has been definitely gained.

It is slowly being learned that those hopes were based on false conceptions of the true nature and mode of action of the various morbific agents. A study of the results of the bacteriology of the urine is exceedingly instructive, for it not only shows us the possibilities and limitations of such investigations, but also rerders more comprehensible some fundamental truths about infectious diseases.

Numerous experimental and clinical studies have been made in this field during the last few years, and in a valuable series of articles in the *Berliner klin. Wochenschrift*, Nos. 7, 8, and 9, 1888, DR. H. NEUMANN, of Berlin, has published a critical analysis of the most important contributions of other writers, besides adding some observations of his own.

The valuable information furnished by the routine

examination of urine, led to the belief that with the aid of simpler means, such as we now possess, much practical information might be obtained as to the nature and course of infectious diseases by a bacterioscopic examination of the urine. This partial truth was based on a widely spread error. It was part of the belief that the atmosphere is "pregnant," as has been said by some, with germs; that these were constantly being swallowed or inhaled, taken into the circulation, and, like the urine salts, "sieved out" of the economy by the kidneys. As a deduction from this bizarre and unfounded idea, it was supposed that in microbic diseases the blood "swarmed" with the specific germs, and, arrived in the renal circulation, they were in turn "sieved out."

Among the first to show the erroneousness of these views was Leube, who found that the urine in health contained no bacteria. More recent observations by Lustgarten and Mannaberg, confirmed by Neumann, show that though this is true of urine in the bladder, the urethra always harbors germs which contaminate it more or less in passing. These bacteria, however, are few in number, and, with one exception, non-pathogenic. The exception is the staphylococcus aureus, and in investigations it can be excluded by the use of sterilized catheters.

Knowing that the urine in health is germ-free, it remains to be seen what changes, if any, occur during infectious disease. At the risk of causing some confusion, it is well to digress here, and consider first the modes of distribution of bacteria in the organism, premising that much of the information on that topic has been gained through studies of the urine.

We already know that in infectious diseases we have to distinguish between local and general infections. Thus in many diseases the germs not only penetrate by the most direct route to the organs concerned, but remain and vegetate there. So in Asiatic cholera, the spirilla remain in the intestine, and no matter how severe the infection, search for the germs in the circulation must be in vain.

Bacterial invasions of the general organism occur in various ways, but for the present purpose may be divided into three groups. In the first the bacteria cause a local infection, and then gain access in a more or less direct manner to the circulation. This has been proven in some cases in suppuration, pneumonia, tuberculosis, and erysipelas. In the second group a lesion is caused in the skin or mucous membrane, and through it an organism which was not the original cause of disease arrives in the circulation. This is a frequent accident in diphtheria, variola, and scarlatina, and in such cases the newcomer is usually a strepto- or staphylo-coccus. In the third group the bacteria course from the beginning in the circulation, or gain access to it at certain periods of the disease. This occurs in recurrent fever, endocarditis, sepsis, glanders, and typhoid fever.

A separate class under this group might include such rare cases as those in which an organism which usually causes only a local infection occurs in the circulation; such cases as those in which the diplococcus pneumoniæ of Fränkel caused meningitis or endocarditis. In such cases it would be fallacious, probably, to infer from an examination of the urine that a latent pneumonia was in progress.

Finally, all kinds of combinations and sequences of these groups may occur in the individual, so that at the same time typhoid bacilli and erysipelas cocci, for example, may be circulating together.

We are now in a position to examine the condition of the urine in cases of blood infection, and first to ascertain if the healthy kidney permits a passage to the bacteria. Notwithstanding many affirmative answers, both clinical and experimental evidence seem rather to deny such an occurrence. Thus, Kannenberg could find spirilla recurrens in urine only after hæmaturia had set in, and numerous culture experiments with the urine of typhoid patients have been negative, though if present the recovery would have been attended with no great difficulty.

A valuable fact that has been learned in this connection is that not all lesions of the kidney occurring in the course of infectious disease are bacterial. According to Neumann's results, severe albuminuria may occur in typhoid fever without bacilli appearing in the urine, and in cases of suppuration even acute nephritis developed with urine examinations negative. These lesions can be explained on the supposition that the excretion of ptomaines causes the nephritis, as Koch has shown possible in the case of the "cholera-kidney."

In order that bacteria be excreted from the kidneys it is necessary that they have accumulated in those organs in large numbers, as happens in glanders, typhoid fever, and pyæmic diseases. Embolism of small vessels or capillaries takes place and then changes occur in the surrounding tissue, varying with the nature of the infectious agent. These

changes are but little understood, though it is clear that multiple emboli, even when accompanied by necrosis and inflammatory foci, do not necessarily lead to inflammation of the whole organ and its corresponding phenomena. Thus, in osteomyelitis with bacteria and round-cells in the urine, albumin and casts were absent almost constantly. In typhoid fever, too, along with bacilli there is sometimes only temporary albuminuria, at others, casts and albumin in large amounts are present.

Existence of bacteriuria may in some cases be recognized without a microscopic examination. Thus, in typhoid fever and some other diseases the bacteria appear to increase in the bladder, the urine when passed appears cloudy and remains so after repeated filtration, and is usually less acid than normal.

The diagnostic importance of bacterioscopic examinations of the urine, aside from cases of primary bacterial nephritis, which we have purposely not considered, depends on the recognition of the time at which bacteriuria appears. On this point but little is known. The ability to show the existence of a "mixed infection" may assume greater clinical importance than it now has.

In typhoid fever, although these examinations have shown nothing positively new, they have confirmed some earlier ideas. Seitz and Neumann found typhoid bacilli in the urine in cases where the eruption was most intense, and they infer, with much ground, a relation between the number of foci of disease and eruption. As far as the diagnosis of typhoid is concerned we are not very sanguine, since there are few doubtful cases in which culture experiments could give the desired information before either the clinical course or autopsy decided the question. As the bacilli remain present in the urine for some time, the diagnosis of a case coming under observation at a late stage might be made from such an investigation.

For prognosis, the ability to discover a renal complication, or a secondary infection, may become very important.

An important practical conclusion may be drawn from the knowledge of the infectiousness of urine in many cases of typhoid fever, and that the source of danger may remain for many months.

Slight as are the clinical gains so far acquired in the lines thus indicated, it must be remembered that the whole subject is a new one, that we are just coming into possession of practical methods, and, bearing in mind the limitations we have made, there is reason to look for greater progress in the future.

On Tuesday evening, the Alumni and students of the Medical Department of the University of Pennsylvania gave a reception to Dr. D. Hayes Agnew, in commemoration of the fiftieth anniversary of his doctorate. Addresses were made by Provost Pepper, Dr. S. W. Gross, Mr. Lincoln (on behalf of the undergraduates), and Dr. Agnew. Subsequently a handsome collation was served. There was quite a large gathering, including a number of the Alumni from a distance. The Provost announced that the University authorities had determined to confer its highest academic honor upon Dr. Agnew at the ensuing commencement.

We are informed that the place of meeting of the American Gynecological Society has been changed to Washington, so as to conform to the meeting of the Congress of American Physicians and Surgeons in that city, on September 18th, 19th, and 20th.

We are in receipt of a circular from Mr. George S. Davis, publisher of the *Index Medicus*, stating that this periodical is not self-supporting and needs a larger measure of aid from the profession than it has yet attained to place it upon a permanent basis. This journal occupies a unique position as a means of research, and the best interests of the profession demand that it be sustained.

The appreciation of its value has extended to Europe, and recent foreign periodicals note with regret that it is in need of aid, and are united in expressions of its great value. We sincerely hope that the American profession will not incur the reproach of neglecting this most important instrument in the promotion of medical research, and we would especially urge every medical society in the country to subscribe for at least a single copy.

We are informed that a national association under the title of "The American Association of Obstetricians and Gynecologists," was formed at Buffalo, on April 19th. It is to be composed of obstetricians and gynecologists, and those surgeons who are interested in the abdominal surgery connected with those specialties. The list of foundation members numbers thirty-four, and includes men from all parts of the country. The initiative meeting was held at the Niagara House, seventeen members being present. The business consisted of the adoption of a Consti-

tution and By-Laws, the election of officers, and the appointment of the place of the annual meeting.

The officers elected were as follows:

President.—W. H. Taylor, M.D., of Cincinnati.

Vice-Presidents.—Drs. E. E. Montgomery, of Philadelphia, and J. H. Carstens, of Detroit.

Secretary.—W. W. Potter, M.D., of Buffalo.

Treasurer.—X. O. Werder, M.D., of Pittsburg.

Executive Council.—Drs. Thomas Opie, of Baltimore; J. H. Etheridge, of Chicago; C. Cushing, of San Francisco; Melancthon Storrs, of Hartford; and Byron Stanton, of Cincinnati.

Delegate to the Congress of Physicians and Surgeons.—James P. Boyd, M.D., of Albany.

Alternate.—Hampton E. Hill, M.D., of Saco, Me.

Place of meeting, Washington, D. C., September 18 to 20, 1888, during the session of the Congress.

During the day the new Association became the guest of the Buffalo Obstetrical Society at a luncheon. The meeting is said to have been a most successful one, and it is the opinion of its founders that the Association is strong, and destined to be a working body, and take high rank among the societies of America.

THE Seventh International Congress of Chemistry and Pharmacy will meet at Milan in September, 1888.

SURGEON-GENERAL MOORE, of the Army, and Surgeon-General Browne, of the Navy, were entertained at dinner at the University Club last Saturday evening by the Practitioners' Society of New York City.

THE Society of Apothecaries of London have determined to permit female students of medicine to come up for examination. A movement is on foot to organize a "Ladies' University."

DR. EDWARD C. W. O'BRIEN, of Buffalo, has been nominated by the Governor of the State of New York for the office of Health Officer of the Port of New York.

THE country practitioner of Great Britain is threatened with a horse-tax of five dollars for each animal he uses. As he is already the subject of a levy upon his carriages, in about an equal sum, this threatened imposition is highly unpopular with a profession which believes itself to be already overburdened.

SMALLPOX was exported from New York City to the Clyde, on the "Circassia," arriving out March 22d. There were five cases, all among the crew. The sick were removed to Greenock Hospital; vaccination and fumigation were thoroughly performed. The steamer was then allowed to proceed to Glasgow. No subsequent cases have been heard from.

SOCIETY PROCEEDINGS.

NEW YORK COUNTY MEDICAL ASSOCIATION,

Stated Meeting, April 16, 1888.

THE PRESIDENT, J. R. MACGREGOR, M.D., IN THE CHAIR.

DR. JOSEPH E. JANVRIN read a paper on

THE PRIMARY REMOVAL BY ABDOMINAL SECTION OF THE TUBE AND ITS CONTAINED FŒTUS IN CASES IN WHICH PREGNANCY HAS BEEN DIAGNOSED, BEFORE RUPTURE OF ANY PORTION OF THE TUBE HAS OCCURRED.

The operation referred to, he said, was one performed with the expectation of delivering the tube and its contained fœtus intact, and done before the end of the fourth month of gestation. In a paper read by him before the American Gynecological Society, in 1886, he had advocated early laparotomy, as a rule, in cases of tubal pregnancy in which hemorrhage had occurred (instead of resorting to the use of electricity), thus removing all possible danger of further hemorrhage. In December of that year he reported, at a meeting of the New York Obstetrical Society, a case in which he had diagnosed tubal pregnancy at the fifth week, and in which death had occurred at the end of the seventh week from a second hemorrhage from a ruptured artery on the peritoneal surface of the sac; the first hemorrhage, which was extremely small, having occurred from this same artery some days previously, and galvanism having in the meantime been employed to destroy the fœtus.

At that time, he said, he was convinced from this case, and from three other similar cases, that the time to operate was when preliminary or partial rupture occurred, as indicated by the first hemorrhage; the primary collapse symptoms constituting an almost certain indication both of the pathological condition and of the need of immediate interference. In speaking of the symptoms of tubal pregnancy, he had stated that he believed it was easier to recognize this condition than a case of hydrosalpinx, and that the presence of an irregular decidual discharge in a woman who had missed a period, associated with the normal signs of pregnancy, and an enlarging, fluctuating, and exquisitely sensitive mass on one side of the uterus, and followed subsequently by symptoms of shock, could hardly be referred to any other condition even as early as the fifth week. In a case of this kind he had also said that he would certainly perform laparotomy whenever the first symptoms of internal hemorrhage occurred.

Again, in a discussion at the January meeting of the Obstetrical Section of the New York Academy of Medicine, in 1887, he had stated that in any case in which from the rational and physical symptoms he became convinced that tubal pregnancy was present, even

before symptoms of a rupture of the peritoneal covering of the tube occurred, he would most earnestly urge laparotomy, and perform it if he could obtain the consent of the patient.

Dr. Janvrin then went on to say that up to the present time he had been able to find but one case on record in which the operation was performed prior to a laceration, or partial laceration, of the tube, and before the fourth month of gestation. This was one reported by Dr. Joseph Price, of Philadelphia, in the *Annals of Gynecology* (Boston), for March, 1888, and in a recent letter to Dr. Janvrin, Dr. Price had said concerning it: "The history was simply that of delayed bleeding two weeks, and constant pain and nausea. I operated for extra-uterine pregnancy or pus tube." He next referred to cases of laparotomy performed in tubal pregnancy, after one or more decided hemorrhages, reported by Lawson Tait, Martin, of Berlin, and others in Europe; and by Drs. A. W. Johnstone, of Danville, Ky.; Joseph Price, of Philadelphia; S. C. Gordon, of Portland, Maine; Wm. Gardner, of Montreal; G. M. Tuttle, of New York; and Charles B. Penrose, of Philadelphia.

All such operations, performed after there had been a rupture of some part of the tube, he thought should be classed as *secondary* operations. So far as he knew, the credit of first advocating the propriety of performing abdominal section and removing the ruptured tube with the fœtus and blood-clots, was due to an American surgeon, the late Dr. Stephen Rogers, of New York, who, as long ago as 1867, urged the claims of this operation in such cases.

The advisability of enlarging this field, and removing the tube with its contained fœtus before rupture has occurred, and during the early weeks of gestation (instead of employing electricity for the purpose of destroying the life of the fœtus), Dr. Janvrin said he had advocated on every possible occasion during the past year and a half. Until very recently he believed that he stood almost, if not quite, alone on this point; but during the last few weeks he had found that other gynecologists also had come to the same conclusion. Thus, in the *American Journal of Obstetrics*, for April, 1888, Mr. Lawson Tait said that, although he had never as yet been called upon to operate until after the sac had ruptured, if in any instance there was a reasonable supposition that there was a tubal pregnancy which had not yet ruptured, he should recommend immediate operation. Drs. Joseph Price and Howard A. Kelly, of Philadelphia, he believed were also in accord with this proposition. In a recent letter to Dr. Janvrin, Dr. Kelly said: "The history of fœtuses which have been destroyed by faradization has frequently been associated with protracted suffering to the mother, and wtth conditions of chronic invalidism."

Dr. R. P. Harris, of Philadelphia, in the *American Journal of Obstetrics* for August, 1886, referred to several cases in which attacks of peritonitis have occurred (where the fœtus had been killed by electricity) several years subsequent to the death of the fœtus, and due to its presence in the abdominal cavity. Mr. Tait also has very recently (*American Journal of Obstetrics* for April, 1888) pointed out the danger of the placenta going on developing enormously after the fœtus had died from natural causes.

Up to the present time, however, the weight of opinion among American gynecologists is decidedly in favor of destroying the fœtus by electricity in some form, in cases in which a diagnosis has been made prior to the fifth month of gestation, and in which there has been no decided hemorrhage from the sac wall. Among the advocates of this measure stand Thomas, Garrigues, Lusk, Mundé and others, of New York, and M. D. Mann, of Buffalo, and each of these, as well as others throughout the country, have had excellent results with it. In the *Annals of Gynecology* for February, 1888, Dr. Mann stated that Dr. Janvrin was able to find only three cases in literature, including his own, where rupture of a vessel caused death without rupture of the sac. Since the publication of his paper before the American Gynecological Society in 1886, however, he had seen another case in which this condition was fully demonstrated at the autopsy. Two other important specimens he had recently seen in cases in which abdominal section was performed. In one of these the fœtus had died, from pressure of blood-clots, at about the end of the third month, and several other attacks of hemorrhage had occurred between this date and the seventh month, when the tube, fœtus, clots, etc., were removed. In the second case, where laparotomy was performed at the seventh or eighth week, there was a history of slight hemorrhage from the sac wall and slight peritonitis for a week or ten days prior to its removal.

In the paper before referred to, Dr. Mann, of Buffalo, had expressed the opinion that if rupture has not occurred electricity should always take the precedence of laparotomy. To this proposition Dr. Janvrin said he would assent, provided an experienced laparotomist could not be found to operate, but not otherwise. With a capable operator at command, he would urge abdominal section just as soon as the diagnosis has been made, and before any hemorrhage from the sac, with its subsequent attacks of peritonitis, could take place. These frequently recurring attacks of slight hemorrhage are exceedingly dangerous to the life of the mother, and when they have occurred, and the time comes when laparotomy *must* be performed, the patient is in bad condition for operation, while the procedure itself is much more complicated than at an earlier period. The same could be said in reference to waiting to perform laparotomy until serious symptoms have supervened, after the fœtus has been killed by electricity. Here, too, there will be more or less adhesions to deal with, and the work to be done will be much more complicated than the removal of a simply enlarged and still unruptured tube containing the fœtus, with no products of inflammatory processes to interfere with its easy extraction.

Dr. W. T. Lusk said that he wished he could be convinced by the arguments advanced in the paper, that the time had come for the doing away with galvanism in the treatment of tubal pregnancy. It would certainly be very gratifying if one could always perform laparotomy as soon as this condition was diagnosticated, as the operator would thus secure any amount of *éclat* which is not possible when electricity is employed. Destruction of the fœtus by the latter is a far less brilliant method of procedure, and when it is attended with successful results the medical attendant is not infrequently subjected to insinuations that extrauterine pregnancy has never existed at all. At the same time he was so thoroughly convinced that these cases can be efficiently and safely treated with galvanism after the

appearance of the characteristic colicky pains, that he did not as yet feel justified in abandoning so satisfactory a method. The operation is a very simple one, requiring no special skill, so that it can be practised by any one, and, as a rule, the results are all that could be desired.

Dr. Lusk went on to say that he had had two of his cases, occurring in private practice, under observation for a long period, one of them for six years, and the other for four years. At the time the first of these patients was operated on it was thought that she was dying. After consultation with Dr. Thomas it was decided to employ electricity, and the result had been entirely satisfactory. There had never been the slightest bad effect, and at the present time the mass remaining was about the size of a pigeon's egg. In the second case, in which there had been the colicky pains and other usual signs of tubal pregnancy, the tumor was now about the size of an English walnut. Since the operation the patient had given birth to a living child, and there had been no trouble whatever at the time it was born.

Now, if we have such an efficient method which is perfectly safe in the great majority of instances, he could see no good reason for giving it up. Even if inflammation, suppuration, or ulceration should ensue, we can usually perform laparotomy then with satisfactory results. He was, therefore, convinced that in the present state of our knowledge this is the best plan of proceeding. As to performing laparotomy after rupture of any portion of the tube has occurred, the difficulty of diagnosis at this time is exceedingly great, and Lawson Tait had told him personally that he did not feel able to make it; or, at least, this was what Dr. Lusk understood him to say.

In regard to the other five cases which he had treated with electricity, he could not speak with the same confidence as of the two just mentioned. The results were satisfactory at the time, but as the women were hospital patients, he had not been able to follow up their subsequent history. If he could be convinced that laparotomy is just as safe as electricity, he should certainly join the ranks of those who advocate this procedure. As to the danger of the growth of the placenta after the fœtus had been destroyed, this seemed to him to be very slight, and he questioned very much whether such a danger really exists at all.

DR. GEORGE T. HARRISON said that his experience with galvanism had been very favorable. He had certainly destroyed the fœtus by this means in three cases. He had examined one of these patients two years after the operation, and had found the sac much shrunken. Since then the woman had given birth to a child in a normal manner. There had also been no evil results in his other cases. Notwithstanding this, however, he thought the experience of the profession is at present too limited to enable us to arrive at any definite conclusions, and the teaching of modern gynecology is undoubtedly to advance in the line indicated by Dr. Janvrin.

The results obtained by some of the German operators especially, have been such as to have great weight in convincing him of the practicability of laparotomy in this connection. Still, at the present time he was not quite willing to declare himself in favor of the general adoption of the procedure in this class of cases. The statement of Dr. Janvrin in regard to the constant occurrence of extreme sensitiveness in the tumor of tubal pregnancy he thought should be somewhat modified, as he had observed instances in which this was not present.

DR. ANDREW F. CURRIER said that such remarks as he had to make on this subject were largely theoretical in character. After a careful consideration of the matter it had seemed to him that it was one of the inconsistencies of modern gynecology, that while the abdomen is constantly being opened for the removal of tumors which may be doing the patient no injury, operators should refuse to do the same for the relief of a condition which, if uninterfered with, is absolutely sure to prove fatal, and should leave the case to the uncertainties of a method like electricity; although in many cases, perhaps most cases, the latter has proved successful. Men, he thought, are apt to be influenced by their surroundings, and if some of those who are the strongest advocates of electricity had met with a fatal result in any of their cases, they would not, perhaps, be so enthusiastic in its favor. Several years ago the late Professor White, of Buffalo, had advanced the proposition (which he thought was a very sound one), that whenever there is in the abdomen an extraneous body which is liable to cause fatal results, the proper surgical course is to open the abdominal cavity and remove it. Again, the possibility of the subsequent growth of parts connected with the fœtus ought not to be ignored, since such men as Tait and Knowsley Thornton have spoken of it as a very serious danger.

When gynecologists perform laparotomy so frequently for ordinary pyosalpinx or hydrosalpinx, why should they hesitate to open the abdomen for so grave a condition as tubal pregnancy? Probably the reason why this matter has not attracted more attention than it has, is because these cases are so seldom seen. If this were the case, it showed the importance of manfully warning those who were to become family practitioners, as well as those who were to become mothers, of the dangers of such an accident, and the necessity for the early recognition of the condition. So far as he was aware, but little attention is paid to the subject in the medical schools.

A. PALMER DUDLEY said that the all-important point seemed to him to be the matter of diagnosis, and he should very much like to know how to make the diagnosis. Even Mr. Tait, according to Dr. Lusk, had confessed his inability to do this before rupture had taken place. If the diagnosis be once established, the treatment is sufficiently easy, whether electricity or laparotomy is resorted to. Under the circumstances referred to by Dr. Janvrin, laparotomy is a comparatively simple operation, and no more dangerous than when it is done for hydro- or pyo-salpinx.

Dr. Dudley then related a case which, he said, showed the difficulty of making a correct diagnosis. The patient was a married woman of seventeen, who was irregular in her monthly periods. She had already had one child. The cervix uteri was soft, and she suffered from nausea and vomiting. There was a pulsating soft tumor, extremely sensitive to the touch, which Dr. Dudley thought was probably due to tubal pregnancy. He proposed laparotomy, but this was opposed, and the patient passed from observation. Two months afterward she returned to the hospital clinic, still suffering from nausea and vomiting, and this time she consented to the operation. This was performed two weeks ago. On opening the abdominal cavity he found pyosalpinx on the

right side, while the tube was twisted around so that its fimbriated extremity was attached to the bladder, behind which the ovary was lodged. The fimbriated extremity of the other tube was attached to the rectum, while under the pyosalpinx was a parovarian cyst. There was a frightful hemorrhage during the operation, but the patient is now recovering. Here, then, was a condition other than tubal pregnancy giving rise to the symptoms mentioned by Dr. Janvrin. In conclusion he said that he would not hesitate to resort to laparotomy early if he was able to make the diagnosis of tubal pregnancy.

DR. LUSK said that in considering laparotomy in connection with pyo- or hydro-salpinx and with tubal pregnancy, we must remember that there is this difference, that while the former conditions cannot be removed by electricity, it is, as a rule, quite possible to put an end to tubal pregnancy by this means. If this were practicable in the case of pyosalpinx also, we would not resort to laparotomy until after electricity had been tried. He then spoke of the importance of arriving at a correct diagnosis in many cases, of making a thorough examination of the patient under ether, and referred to two cases in his own experience illustrating this point.

DR. DUDLEY stated in reply to a question by Dr. Janvrin that in the case which he had related he had not observed an irregular decidual discharge.

DR. JANVRIN said that he regarded this as the most important point in the diagnosis of tubal pregnancy. If the symptoms which he had mentioned (leaving out those of shock, which is due to rupture) were noted in any case, he thought there would be no difficulty in making out the diagnosis before rupture of any portion of the tube had occurred. These, it should be remembered, are the presence of an irregular decidual discharge in a woman who has missed a period, associated with the normal signs of pregnancy and an enlarging, fluctuating, and exquisitely sensitive mass on one side of the uterus.

Dr. Lusk had stated that after the employment of electricity there is very little danger of ulceration and other bad effects. This is no doubt true, as a rule, but certain cases have been reported in which it was followed by evil results. Thus, Dr. Mann has published in the article referred to in the *Annals of Gynecology*, two such cases after the use of galvanism. As to Mr. Tait's views on the diagnosis of tubal pregnancy before rupture, in the very last number of the *American Journal of Obstetrics* (April, 1888) he stated that he had never been able to make the diagnosis at this time because he had never been called in any case until after rupture had occurred. The German cases referred to by Dr. Harrison were all secondary operations, and, so far as he was aware, the only primary operation which had as yet ever been performed was that of Dr. Joseph Price, of Philadelphia, which was done in September last.

THIRD FRENCH CONGRESS OF SURGERY.

Held in Paris from March 12 to 17, 1888.

(Specially reported for THE MEDICAL NEWS.)

(Continued from page 446.)

EXCISION OF THE WRIST FOR ANKYLOSIS.

DR. OLLIER, of Lyons, reported two excisions of the wrist for ankylosis. These operations would never have been undertaken, he said, had the antiseptic methods not been used, and a sufficient functional restitution of the part assured. It would not have been accepted a few years ago, when ankylosis was the ideal result sought for in excisions. Langenbeck had tried it twice, but in one case he had pyæmia and in the other a return of the ankylosis.

At the present time, after the excision of the carpus in an isolated manner or associated with that of the metacarpal or ulnar-radial extremities, one can obtain excellent functional results, far superior to those obtained by Lister in 1865, which, however, showed already at that time a marked improvement. One of the most important results obtainable is a joint movable by communicated movements, and also self-moving; we arrive at that by respecting the motor-tendons of the wrist, radial and ulnar, which were formerly sacrificed. Of course, the articulation has not its normal freedom, but it will have movements of at least 40 to 45 degrees, which is quite sufficient for the ordinary use of the wrist. Having seen, four or five years later, two autopsies of resected patients, he found between the extremities excised an osteo-fibrous cushion with synovial interstices, giving a good functional condition to the articulation.

The possibility of writing is not a good test, for one with stiff fingers can do this; all his patients can hold their arms out with a weight of 10, 24, and 36 pounds while moving their wrists, the movements of abduction and adduction of the thumb are also well restored. In cases of ankylosis the operation is always a delicate one, especially when the wrist is S-formed. The carpal bones must then literally be carved with the chisel and mallet with great care not to wound any arterial or nervous trunk. In rheumatic patients, on account of a certain degree of tendinous ankylosis, one must take a good portion of the bone away. The operation, if the trouble is not accompanied by tuberculous lesions, must also be absolutely without subsequent fever. The first case he operated upon was a young woman who had already a double osseous ankylosis of the elbow, and he excised the carpus and about one-third of an inch of the bones of the forearm to allow her to play her piano, which she now does very readily. His second case was of ankylosis giving a bad position to the hand, rendering it absolutely useless. The functional result obtained is a perfect one.

EXTIRPATION OF THE LARYNX FOR CARCINOMA.

DR. DEMONS, of Bordeaux, did not think that on account of its difficulties and the rapid return of the disease, if the patient does not die from the operation, this operation ought to be disregarded. He reported two cases. The first case, man, aged forty-four years, had presented for one year signs of cancer of the larynx. He had an epithelioma which started at the epiglottis, then invaded the base of the tongue and the pharyngeal wall with bilateral glandular swellings. Tracheotomy had been performed, and the patient asked for a radical operation. He first took away, by a transverse incision below the level of the hyoid bone, the epiglottis and the base of the tongue, then by a median vertical incision, the left lateral half of the larynx. At the end of nine months the disease returned at the base of the tongue. The second case, man, aged fifty-seven years, had had for one year laryngeal symptoms, and presented a limited

epithelioma on the right vocal cord. This operation he made last May. The epithelioma was found limited to the larynx without any lesions of the cartilages or lymphatic glands. He first made a transverse incision below the hyoid bone from the middle of which he started a vertical incision down to the larynx. He incised the thyroid cartilage vertically and finding the left side also slightly involved, he extirpated the whole larynx. A month later the patient was cured, and for ten months has felt perfectly well, and there is no sign of the return of the trouble. The diagnosis was confirmed by the microscope. He concluded, that if the affection takes a rapid course we should refuse to operate, but, if, on the contrary, the disease is limited, we should not hesitate to use the knife.

DR. DUPONT, of Lausanne, drew attention to a modification of the operation. The principal danger of the operation being the introduction of blood into the air passages, the Trendelenburg canula is used by some operators. He separates the trachea from the œsophagus and brings it forward, fixing it at the inferior angle of the wound. In that manner the trachea is no longer in the direction of the inferior extremity of the larynx, and the blood falls behind it.

DR. MOLLIÈRE, of Lyons, had extirpated the larynx in a man of sixty-eight years, for an epithelioma of the arytenoid region. He excised the posterior one-third of the larynx and the greater part of one of the vocal cords, which he then tried to restore by sutures. He then finished by suturing the epiglottis to the cutaneous wound. To prevent the passage of food into the larynx, Trendelenburg's canula was used with good success, and was left ten days in the wound, until it was cicatrized. It has been much better borne than the ordinary canula on account of its not moving and being elastic. The patient was nourished with the œsophageal sound for two days and ate alone on the third day. He left the hospital at the end of one month.

WHAT TO DO IN GUNSHOT WOUNDS OF THE VISCERAL CAVITIES.

DR. CHAUVEL, of Paris, said that on this subject the profession is divided into two camps: the temporization party and the intervention party; but the introduction of antiseptic surgery ought to modify the old views. In the cranium and chest the infection of the wounds of the serous cavities can only come from the exterior; in the abdomen it comes from the wounding of the abdominal organs, which pour their products into the peritoneum. For the past hundred years immediate intervention has been defended by Thomassin, Dufouart, Baudens, Legouest, Otis, etc., but their precepts were not followed. In France the opinions are varied, the abstentionists are represented by Professors Tillaux and Verneuil; the moderate interventionists by Professors Trelat and Reclus, who operate as soon as signs of peritonitis appear, and indicate a peritoneal effusion of some kind; to these later views adhere also MM. Bouilly and Le Fort.

Hamilton and MacCormac recommend immediate laparotomy, and he adheres to these later views. He thought that any wound of the abdomen should be followed by an immediate enlarging of the wound, exploration of the parts, etc. Gunshot wounds are almost always mortal, if we rely on expectation. Dr. Nimier in his statistics had found the mortality to be variable; in certain cases ninety-five per cent. die, while others lose about ten to fifteen per cent. He has collected five thousand and three cases, the mortality of which was eighty per cent. In gunshot wounds of viscera, the line followed by the bullet is always a direct one, and the wounds are always according to a direct line from the wound of inlet to the one of outlet.

Intestinal movements may produce a slight deviation from this line, but not a considerable one; this enables us to find out more easily the injuries sustained.

At the beginning the two pathognomonic signs are very obscure, the appearance of the intestinal contents at the wound and visceral hernias are rare; still, one cannot await the general symptoms. Death usually occurs by hemorrhage or by septic peritonitis. The hemorrhage may be due to a large vessel or again to the tearing of small arterioles. Septic peritonitis is the result of the presence of intestinal matters in the peritoneum. Hemorrhage and peritonitis being the principal causes of death, is it not natural that these should be prevented by the ligature of the wounded vessel, and by the cleaning out of septic matter?

Out of forty-one cases of laparotomy for gunshot wounds of the abdomen by small bullets since 1880, one finds twelve cures and twenty-nine deaths. If one should eliminate the cases in which the operation had been undertaken too late, the proportion of cures would be much greater.

The washing out of the peritoneum, proposed by Nimier, cannot replace laparotomy. This operation must be done in the median line, and as rapidly as possible. The extraction of foreign bodies, ligature of vessels, suture of intestine before or after excision of the same, the washing out of the cavity, the extirpation of certain organs, will be practised by incision in the median line. Finally the medical treatment: absolute rest, opium, fasting, will help the surgeon. One must then reject systematic expectation, and operate immediately.

DR. E. DE LORME, of Paris, said that direct surgical intervention in intestinal wounds by gunshot is a very rational operation, but practically immediate intervention for gunshot wounds of the abdomen on the battlefield is not at all to be recommended. These operations could only be undertaken in fixed ambulances which are not obliged to follow the army.

The principal contraindications are: 1. The wounds produced by voluminous pieces of shell, which produce very extensive damage which can hardly be remedied by surgical aid. 2. The multiplicity of the wounds of the small intestine by bullets which penetrate the abdomen; still this is a less serious objection on account of the progress made in intestinal suture. From a series of experiments on cadavers, he has found that in transverse perforations of the intestine the intestinal lesions are much more numerous than in antero-posterior perforations. 3. When the bullet has a very great speed, the frequent extension of the lesions contributes to an unfavorable prognosis, and renders less certain direct intervention.

Gunshot wounds of the abdomen in a campaign are in a bad condition for laparotomy. The operation cannot be performed in the field ambulance, and when the wounded are admitted to a permanent hospital it is too late.

DR. PONCET, of Paris, said that in the time of Larrey cannon balls were round, and would produce by rub-

bing against the abdomen severe bloody effusions in the abdominal cavity. Later, in 1855, the explosive shells produced contusions without ecchymosis of the parietes, but intestinal lesions. He had seen during the siege of Strasburg, cases in which contusion of the abdomen produced by large pieces of shell would only present symptoms of intestinal perforations by sloughing at the end of the seventh or eighth day. Formerly the shells were made to explode according to certain lines, indicated by weak points at the time of the manufacture of the shell; but one of the shells in present use will explode in four or five hundred pieces; in addition we have the metallic dust which accompanies such explosions, and which is capable of penetrating the tissues to the depth of four inches. These are only the effects of our cast-iron shells. But what will the wounds be produced by steel shells; they are still more dangerous and produce terrific wounds. All the splinters of the shell are each like a knife cutting on both sides, which are thrown on the human body with such force that they are found at three hundred and fifty yards behind, or nine hundred and fifty yards in front of the point of explosion, and so hot that they cannot be touched. Such are the shells which will produce the wounds of the next war; as to say what kind of wounds they will be and what to do, we must wait experience. In every case of immediate perforation laparotomy ought to be performed, and suture and excision of the intestine made. If there is a consecutive perforation after elimination of a suture, a secondary laparotomy will have to be performed.

Dr. Reclus, of Paris, said that American and English English surgeons whenever they find themselves in the presence of a perforating wound of the abdomen produced by a revolver, make an immediate laparotomy. For them intestinal perforation is a necessity to a perforating wound, and death is inevitable if an intestinal wound is left to nature. He accepted the first proposition, for in only one case out of thirty-seven had he seen a gunshot wound of the abdomen not followed by perforation of the intestine. But he did not admit the second proposition that perforation of the intestine always causes death. This opinion has been established in two ways: 1. The confusion which has been established between revolver wounds and those produced by war rifles, which last are much more dangerous. 2. The results of experiments on dogs. All the animals on which Parkes experimented died. But the dog is not in the same condition as man, on account of the shortness of the intestinal canal, and of the relatively larger size for him of a revolver ball, which are all conditions favorable to an escape of the intestinal matters into the peritoneum. 3. Maintaining in cases of abdominal wounds terminating by cure that there did not exist any intestinal wound, cure in these conditions is, however, frequent enough. He could collect more than one hundred such cases where the perforation had been recognized by the usual symptoms: fecal matter coming out of the wound, immediate hematemesis, melæna, or early expulsion of the ball by the anus.

Three principal causes bring on death in abdominal gunshot wounds: hemorrhage from a large vessel, shock, and peritonitis from effusion of intestinal matter. Against the first two very little can be done; against the third, much more frequent, intervention is not always necessary. Nature will prevent, in several ways, the intestinal contents from being effused in the peritoneum: 1. By the formation of a mucous obturator which closes the perforation. He has verified this fact in his experiments on dogs. 2. By a peculiar way, the simple contraction of the muscular coat, which will close the opening. 3. The most common, by the rapid formation of adhesions with a neighboring intestine or with the parietal peritoneum. In these abdominal wounds he acts according to the following rules.

1. Strong compression of the abdomen, as after laparotomy, to prevent an effusion of intestinal matter or blood into the peritoneum.

3. Opium, and absolute fasting, giving only every quarter of an hour a little iced milk to allay thirst.

3. It is only then, when these precautions have failed, that laparotomy should be resorted to.

Dr. Trelat, of Paris, was absolutely opposed to the two doctrines, 1st, the absolute operative abstention, which is condemned by six or seven examples of certain cures due to intervention; 2d, the expectation, until sure signs of peritonitis have set in. He thinks laparotomy necessary as soon as there are signs of stercoraceous effusion. These phenomena may be immediate or only appear later, but as soon as there is the least sign of peritonitis, he would not hesitate to operate. The medical treatment recommended by M. Reclus is advantageous, but must not be entirely relied upon.

Dr. Labbé, of Paris, recorded a case of a man, of twenty-nine years, alcoholic and addicted to morphia, who shot himself in the abdomen with a revolver-ball, size ⅓ of an inch, directed from above downward. He saw him two hours later, with hardly any pulse, cold, having had already several fainting-spells, and appeared lost. A few injections of ether restored him, however, somewhat, and he having shot himself in the country it was necessary to take him to Paris, about three miles, where he arrived about six hours after the accident, his temperature 96° F., no immediate intervention could be thought of. During the night he had violent pains, green vomiting, temperature raised. In the morning slight tympanites indicated peritonitis. He operated upon him fourteen hours after the accident. On incising the abdominal wall about three pints of blood escaped; and on examining the small intestine he found about the middle of the ileum four double perforations very near to each other, and above them a fifth one; in addition the mesentery was perforated, and was the source of the hemorrhage. The intestines were closed by Lembert sutures, the peritonenm washed out with sponges and great quantities of boiled water, and the abdomen closed. Operation lasted one hour and ten minutes. After the operation the patient became warmer, but died in twenty hours. This indicates in such cases immediate intervention, for in fourteen hours the peritoneum had had time to become red and have exudations on its surface.

Dr. Nimier, of Paris, said that from the study of gunshot wounds of the cranial cavity, during the Tonkin War, he found that most of the deaths were caused by the irritation of the cerebrum by the ball, the blood, bony splinters, etc., or by infection of the wound. Cure had especially been obtained in those cases where an early intervention had suppressed these inflammatory causes. He recommended in cases of gunshot wounds of the cranium: To shave and clean the cranium; to cut away the bruised lips of the wound; to enlarge it, if necessary,

to elevate the bone or take away the bony splinters; take away the foreign bodies, or the apparent missiles; work with an antiseptic solution, then dress with an occlusive antiseptic dressing, If there exists a hernia of the cerebrum, as in one of our wounded who was cured, the surgeon must apply a slightly compressive bandage. If the ball is lost in the cranial cavity, some symptom may indicate its location; intervention becomes then a matter of diagnostic science or pluck. In case of meningo-encephalitis the surgeons who have an opportunity might try the opening of the serous cavity, and the antiseptic irrigation of the same.

SPONTANEOUS HEMATOMA OF THE ORBIT.

DR. PANAS, of Paris, last January examined a boy, four years old, habitually healthy, except that he suffered from repeated epistaxis, and presented in the left orbicular region, upper and external part, a lobulated tumor, which had pushed the eye out, downward and inward. Ophthalmoscopic examination showed that the optic papilla was white and diffuse, while the luminous perception was nil. The orbital tumor presented a certain fluctuation, and punctures brought out black and clotted blood; it was a pure hematoma. It was only after four months that the superior region of the orbit began to disappear gradually, but one of the important symptoms exhibited was very serious dyspeptic troubles, real indigestion, accompanied every time by epistaxis; and this dyspeptic phenomenon even went as far as vomiting. Considering the general condition of the patient, we find him too stout for his age, that he is a dyspeptic, and a fatty dyspeptic. He thinks that this orbital hematoma is connected with the dilatation of the stomach; it is well known that congestion or hemorrhage is very common in stout dyspeptics. Is there here a phenomenon of reflex hemorrhage of the order of vaso-paralytic manifestations?

ON CHOLECYSTECTOMIES.

DR. THIRIAR, of Brussels, said that at the first session of this Congress he presented three cases of ablation of the biliary vesicles; his three patients are still well today, and have had no return of hepatic colic. In July, 1887, in a lady aged thirty, he extirpated the biliary vesicle. She had suffered for six years from terrible hepatic colics, which nothing could relieve. The vesicle contained no calculi, was enclosed by the liver and duodenum, to which it adhered firmly. The dissection was a difficult one, as in certain parts the duodenum was deprived of its serous tissue. Finally, the vesicle was extirpated. The cure was rapid, and the patient has had no return of the trouble.

Out of 6 cases of cholecystotomies by Spencer Wells's methods, there have been 3 deaths, 50 per cent., and one return of the trouble; out of 72 cases of ordinary cholecystotomies—that is, with suture of the biliary vesicles on the abdominal walls—13 deaths, or 18.05 per cent.; 59 operated upon have survived. There has been one return, and it had a persistent fistula. We had, then, 53 radical cures, which gave 48.6 per cent. of complete cures.

We know of 22 cases of cystectomies with 3 deaths after the operation, which makes 13.65 per cent. Of these 3 failures, one is attributed to a cerebral tumor; the second was a perforation of the cystic canal by a calculus. In the third case there were two calculi in the ductus communis choledochus, one was pushed back, the other crushed, according to Lawson Tait's method.

Cholecystectomy gives us, then, leaving the first case out, a mortality of 9.98 per cent., and all the operated patients were cured. In accordance with these results he thinks we must interfere without hesitation. The biliary vesicle is of no use in the economy, it can be extirpated without inconvenience, and if we could demonstrate the existence of a painful cholecystitis, or irritable vesicle, as we have painful cystitis and irritable cystitis, the indications of cholecystectomies could be very much extended, one would have to resort to it in all painful crises having for their origin an irritable biliary vesicle.

OBITUARY.

CORNELIUS REA AGNEW, M.D., LL.D.

THE profession in New York was very much startled on Monday, April 8th, to learn that Dr. Agnew had gone home from a consultation with serious abdominal symptoms. These soon developed into what appeared and proved to be general septic peritonitis.

Dr. Agnew had generally been in excellent health; he had always taken wise and good care of his physical system. He came of a long-lived family; he was not fifty-eight years old when he died, his birthday being August 8, 1830. His appearance of excellent bodily and mental preservation had given his friends reason to think that he would live out his days, and fill to the full the measure of his useful and distinguished life. But he succumbed, after an illness of eight days, to a disease that probably originated in the appendix vermiformis, for while no foreign body was found, impacted feces and perforation and peritonitis started from this point.

Dr. Agnew was born in New York City, and was graduated in the arts from a New York college, in medicine from the College of Physicians and Surgeons, after which he served the full terms of assistant and resident in the New York Hospital. His educational lineage was of the best type. He began practice as a general practitioner, with a strong leaning to surgery. At one time he was talked of as attending surgeon to the hospital where he was educated. He soon, however, devoted his entire professional energies to the care of diseases of the eye and ear, for which he had especially fitted himself by study in Paris under Sichel and Desmarres, in London under Bowman, and in Dublin with Sir William Wilde. He never became a narrow specialist, and to the end of his days he would have been a competent general practitioner, so wide were his experience and his studies.

His special work in diseases of the eye was begun in the New York Eye and Ear Infirmary in 1855. He resigned this position in 1864. His services in connection with Van Buren, Gibbs, and Bellows, in establishing and carrying on the Sanitary Commission during the Civil War, are a part of our national history. He was one of the founders of the Union League Club of New York, and an honorary member of it at the time of his death. He was chiefly instrumental in establishing the Brooklyn Eye and Ear Hospital in 1869. By his influence such citizens as S. B. Chittenden and H. B. Claflin were induced to found and finally to endow this charity.

In 1869, in connection with Drs. Loring and Roosa, he engaged the interest of prominent and wealthy citizens

of New York, so that the Manhattan Eye and Ear Hospital was established in a dwelling-house hired for the purpose. Through Dr. Agnew's untiring and skilful efforts this institution was also permanently settled in a large and handsome building erected for it. Among the chief of the leading New Yorkers who contributed the quarter of a million of dollars finally necessary to this end, was the Honorable Edwin D. Morgan, on whose staff Dr. Agnew acted as Surgeon-General in 1859 and 1860.

Dr. Agnew did a large share of the surgical work at the Brooklyn Eye and Ear Hospital for a number of years, and in the Manhattan up to the moment of his fatal illness. He was easily among the first as an operator, and he also possessed great skill in the general management of cases, and ability in the executive care of hospitals.

He was a devout member of the Presbyterian Church with strong dogmatic convictions, but he was extremely catholic and liberal in his judgments of those whose theories of life and its duties were different from his. He was preëminently a peacemaker in the profession, in all the little difficulties and misunderstandings that arise among bodies of men with the same interests.

His contributions to medical literature are by no means the least part of his work. They are to be found in our medical journals and in the Transactions of the American Ophthalmological and Otological Societies. They were of such a nature as to be generally quoted and often translated into the German and French languages. His reputation as an ophthalmologist and otologist is as wide in Great Britain and Ireland and on the Continent of Europe as in our own country.

Dr. Agnew was a man of rather below medium height, but of most dignified presence, a graceful and interesting speaker, and a capital presiding officer. In no part of his life was there any failure. He died in the midst of the active performance of duty. He preserved to the last conscious moment the equanimity and faith that had characterized his life. He gave the most careful directions in preparation for the operation that was performed upon him, in the hope of finding an obstructed bowel. He departed this life, surrounded by his family, in the most intrepid and cheerful spirit, without fear and without reproach.

At a meeting of the New York Academy of Medicine held April 19, 1888, the following preamble and resolutions were unanimously adopted, and, as a further mark of respect to the memory of Dr. Agnew, the Academy thereupon adjourned.

The New York Academy of Medicine has been informed of the death on Wednesday, April 18, 1888, of Dr. Cornelius Rea Agnew, who, both as an officer and a member of the Academy, has been most assiduous and successful in his efforts for its welfare.

The Academy hereby records its appreciation of his untiring zeal, of his eminent ability, of his executive capacity, and of his fidelity; of the largeness of his views for its development, of his generosity both in his gifts and in personal labors for its good.

He was alert to make the best of its methods of work, as well as to improve their efficiency. He sought to develop its scientific discussions and proceedings by judicious classification of subjects; he was warmly interested in the enlargement of the library; he was exacting in attention to economy in the expenditure of its funds; he was deeply earnest in the plan for procuring a new building. In his loss the Academy has sustained a heavy blow, and the removal of his friendly and energetic personality is a great bereavement, while we shall miss his wise counsels, his discriminating judgment, his courtesy and vigor in debate, and his promptness in forwarding every scheme for the scientific and practical advantage of the Academy.

We have lost a true friend and an efficient worker. Beyond what he has done for the Academy, we are proud of his achievements in what makes the medical profession an honor and a power for good.

He founded two eye and ear hospitals and placed them on a successful basis. He aided in the organization and was for many years one of the managers of an asylum for the insane. He was active in many medical societies. While practising a specialty, his outlook and concern were for the whole profession.

He had no professional narrowness, and his judgments were always inclined to be charitable to those who differed with him. His services as a patriot are part of the history of our country; he was zealous for the civic weal, and a leader in movements for reform.

Sanitary and social improvements were among the causes to which he earnestly devoted himself. He took an active part in helping on education, both in its popular cultivation and in its higher and technical developments.

Probity, uprightness, unselfishness, and courage were typified in him, while his character was imbued and moved by deep religious conviction. His life is, to us, a glory and a stimulus.

Resolved, That we respectfully tender to his family, the assurances of our deep sympathy and with the tribute of our affection, our high admiration for the many noble qualities which were embodied in Dr. Agnew.

NEWS ITEMS.

Preliminary Programme of the Congress of American Physicians and Surgeons, to be held in Washington, September 18, 19, and 20, 1888. The *American Surgical Association* announces the following papers to be read:

Excision in Articular Disease. By Dr. John Ashhurst, Jr., of Philadelphia.

Microörganisms: their Relation to Surgical Disease. By Dr. Nicholas Senn, of Milwaukee.

Cerebral Localization: Its Relation to Operative Surgery. By Dr. Stephen Smith, of New York.

Intracranial Tumor Successfully Removed. By Dr. W. W. Keen, of Philadelphia.

Experimental Researches on Gastric Fistula, and its Bearing on Gastrostomy. By Dr. J. Collins Warren, of Boston.

Forty Years of Chloroform and Ether. By Dr. D. W. Yandell, of Louisville.

The Surgical Management of Typhlitis and Perityphlitis. By Dr. W. T. Bull, of New York.

The Choice of Operation for Vesical Calculus. By Dr. Hunter McGuire, of Richmond, Va.

Hernia: Its Treatment, etc. By Dr. C. H. Mastin, of Mobile, Ala.

The Radical Cure of Hernia. By Dr. George W. Gay, of Boston.

Certain Indications for Trephining, and their Comparative Value. By Dr. J. Wm. White, of Philadelphia.

Nerve Stretching. By Dr. N. P. Dandridge, of Cincinnati.

Nephrectomy for Gunshot Wound of the Kidney. By Dr. DeForest Willard, of Philadelphia.

The *American Association of Genito-Urinary Surgeons* announces the following papers to be read:

Clinical Observations on Diseases of the Testicles. By Dr. L. B. Bangs, of New York.

Clinical Observations on Chronic Gonorrhœa, and Two Cases of Cancer of the Seminal Vesicles, with pathological specimens. By Dr. J. P. Bryson, of St. Louis.

Operative Treatment of Hypertrophy of the Prostate; and Case of Bowel Ending in the Urethra of a Child Four Weeks Old; Relief by Operation. By Dr. A. T. Cabot, of Boston.

On the Effects of Rapid Changes of Altitude in an Advanced Case of Interstitial Nephritis. By Dr. George Chismore, of San Francisco.

The Connection between Masturbation and Stricture. By S. W. Gross, of Philadelphia.

Operations on the Kidney. By Dr. W. H. Hingston, of Montreal.

Syphiloma of the Vulva. By Dr. J. N. Hyde, of Chicago.

The Curability of Urethral Stricture by Electricity, and Investigations; and The Comparative Value of Suprapubic and Perineal Drainage in Curable and Incurable Bladder Disease. By Dr. E. L. Keys, of New York.

The Filaria Sanguinis Hominis in the United States, Especially in its Relationship to Chylocele of the Tunica Vaginalis Testis; and A Case of Perineal Section for Traumatic Retention; Unusual Condition of the Bladder. By Dr. J. E. Michael, of Baltimore.

The Prophylaxis of Syphilis. By Dr. P. A. Morrow, of New York.

Unusual Case of Urethral Calculus. By Dr. H. G. Mudd, of S. Louis.

On the Radical Cure of Stricture by Dilating Urethrotomy; and Demonstration of a Perfected Evacuator, and an Improvement in the Method of Removal of Débris from the Bladder. By Dr. F. N. Otis, of New York.

Pyæmia as a Direct Sequel of Gonorrhœa. By Dr. R. Park, of Buffalo.

Retrojections in Gonorrhœa. By Dr. E. R. Palmer, of Louisville.

Prostatotomy for Enlarged Prostate at the Age of Forty-two. By Dr. Abner Post, of Boston.

A Case of Removal of Both Testicles for Recurrent Carcinoma; and A Case of Nephrolithiasis Complicated with Hydronephrosis in which Lumbar Nephrotomy was Performed. By Dr. F. W. Rockwell, of Brooklyn.

Some Points on the Differential Diagnosis of Bladder and Kidney Affections with Demonstrations of the Cystoscope and other Instruments; and On the Physiology of the Bladder. By Dr. Alexander W. Stein, of New York.

Local Treatment of Chronic Urethral Discharges. By Dr. F. R. Sturgis, of New York.

Some Points on the Etiology of Stricture of the Urethra. By Dr. R. W. Taylor, of New York.

Operative Treatment of Hypertrophy of the Prostate; and Spontaneous Fracture of Stone in the Bladder. By Dr. F. S. Watson, of Boston.

The Relation of the Prostate to Chronic Urethral Discharges; The Value of the Tolerance of the Iodides as a Diagnostic Test of Syphilis; and Urethral Stricture and Enlarged Prostate in their Relation to Vesical Calculus and Calculous Pyelitis, with Cases. By Dr. J. William White, of Philadelphia.

The Prognosis of Stricture, based on Thirty Years Death Record of Stricture at the London Hospital and the Practice at St. Peter's Hospital. By Dr. E. Hurry Fenwick, of London.

The Congenital Anomalies of the External Urethral Orifice. By Dr. C. Kaufmann, of Zurich.

Death of Dr. Edward G. Loring.—We regret to record the sudden death of Dr. Loring in New York City on the evening of the 23d inst., presumably from heart disease. Dr. Loring started out in his usual health, and when returning late in the afternoon dropped in the street and died almost instantly.

Dr. Loring graduated from the Harvard Medical School in 1864, and subsequently entered upon the active practice of ophthalmology. He was a member of the American Ophthalmological Society, a frequent contributor to the literature of his specialty, and the author of a text-book on Ophthalmoscopy.

OFFICIAL LIST OF CHANGES IN THE STATIONS AND DUTIES OF OFFICERS SERVING IN THE MEDICAL DEPARTMENT U. S. ARMY, FROM APRIL 17 TO APRIL 23, 1888.

STERNBERG, GEORGE M., *Major and Surgeon.*—Having been instructed by the President to proceed to the Island of Cuba, for the purpose of continuing his investigations of the methods of preventing the spread of epidemic diseases, will in connection with his present duties, report to the Secretary of the Treasury for further instructions.—*S. O 89, A. G. O.*, April 18, 1888.

The operation of par. 17, S. O. 79, c. s., A. G. O. (so much thereof as relates to GEO. C. BUSHNELL, *Captain and Assistant Surgeon*, U. S. Army) is suspended until May 1, 1888.—*S. O. 90, A. G. O.*, April 19, 1888.

ARTHUR, WM. H., *Captain and Assistant Surgeon.*—Ordered from Fort Niagara, N. Y., to Fort Bowie, Arizona, to take effect on the expiration of his present leave of absence.—*S. O. 86, A. G. O*, April 14, 1888.

BLACK, CHARLES S., *First Lieutenant and Assistant Surgeon.*—Ordered from Fort Davis, Texas, to Fort Sidney, Nebraska.—*S. O. 86, A. G. O.*, April 14, 1888.

CROSBY, WILLIAM D., *First Lieutenant and Assistant Surgeon.*—Ordered for duty at Jefferson Barracks, Mo., after being relieved by Assistant Surgeon William H. Arthur, and upon expiration of the leave of absence granted him in S. O. 60, A. G. O, March 14, 1888.—*S. O. 86, A. G. O.*, April 14, 1888.

OFFICIAL LIST OF CHANGES IN THE STATIONS AND DUTIES OF MEDICAL OFFICERS OF THE U. S. MARINE-HOSPITAL SERVICE, FOR THE TWO WEEKS ENDING APRIL 21, 1888.

HUTTON, W. H. H., *Surgeon.*—To proceed to Biloxi, Miss., on special duty, April 21, 1888.

LONG, W. H., *Surgeon.*—Granted leave of absence for fourteen days, April 21, 1888

SAWTELLE, H. W., *Surgeon.*—Granted leave of absence for seven days, April 21, 1888.

URQUHART, F. M., *Passed Assistant Surgeon.*—Granted leave of absence for seven days, April 10, 1888. To assume temporary charge of Cape Charles Quarantine Station, April 17, 1888.

WHITE, J. H., *Passed Assistant Surgeon.*—Relieved from quarantine duties at Sapelo Station, April 21, 1888.

WILLIAMS, L. L., *Passed Assistant Surgeon.*—Relieved from duty at Marine-Hospital, Boston, Mass.; to assume charge of Cape Charles Quarantine Station, April 17, 1888.

BRATTON, W. D., *Passed Assistant Surgeon.*—Relieved from duty at Marine-Hospital, San Francisco, California; detailed as medical officer Revenue Steamer "Bear," during summer cruise, April 19, 1888.